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CONTINGENCY.

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THE conception of contingency has never received such attention from logicians as the related conceptions of necessity and freedom. This, no doubt, is only what is to be expected since, modern philosophy having been until recently so predominantly rationalistic and monistic, contingency has presented itself mainly as something to be explained away. It seems, on the face of it, to have associations with chance and accident and a philosophical system whose leading motive is the "quest for certainty" could not be expected to look with any favour upon anything that would destroy the singleness and the rationality of things. It is rather in semi-philosophical and social theory that considerations of accident and contingency have loomed large, and especially in attempts to establish a philosophy of history or of politics, where the question of inevitability has been much discussed. It has there been considered a problem to decide how far determination, and the sort of necessity that science seems to support, is compatible with human interference with or control of the course of events. If the future is completely determined, if there are no "loose points" in the chain of natural events, must we not say that men are deluding themselves in imagining that at any moment they can intervene and guide the further development of things? It was, of course, the popularity of Hegelian monistic views that helped particularly to create a problem of human initiative, and the position has been made all the more obscure by the fact that the leading *revolutionary* theory of modern times itself sets out from the basis of the Hegelian monism.

It has, however, been possible seriously to ask and to debate such questions because of the theory of necessity that has been held and because, in particular, necessity and necessitation have been taken as excluding contingency. It is true that a number of venerable theories have professed to recognise the reality of the contingency of events, at least as a starting-point; it was, we may say, one of the merits of Leibniz as a logician that he tried, however unsuccessfully, to provide for contingency. But, as has been said, in most cases contingency has been accepted only to be immediately transcended or removed. Thus, one of the classical arguments for the existence of God is from the contingency of events, and a similar type of argument appears in Hegel where it is said that it is the initial irrationality of the world that compels us to rationalise it. This particular proof of God's existence does not, of course, stand examination. What we appear to mean by the contingency of events is that each event we experience is contingent upon others; that for everything that occurs, there are necessary conditions of its occurrence. But how we are to argue from " X is contingent upon Y" to "therefore, X is contingent upon God," or "the world is contingent upon God" is never made clear; indeed, if it is really the case that it is Y that X is contingent upon, that would suggest that our conclusion is really a contradiction of the premise from which we begin.

We notice briefly this argument based upon contingency because it serves to introduce the first feature of contingency that we have to stress. It will be seen that contingency has been accounted for in terms of creation; the world is contingent because it is dependent upon God's decision to create. But it is not difficult to show that this is not what we mean by contingency at all. If we recognise contingency as an actual feature of things, we do not mean that we recognise that the total system of things or "world" is contingent upon something outside it, but that each event is contingent upon some other determinate event within our experience. In other words, we realise that the relation holds, not between the "world" and its creator, but between finite events within the spatio-temporal system. It is for this reason that Leibniz's elaborate theory of contingency cannot be accepted. Leibniz has sufficient penetration to see that we cannot, as Hegel does, simply ignore the relations of contingency which we are everywhere, in our practical activities, taking account of, but at the same time he holds to the metaphysical theory of essence, according to which each successive state of a subject develops out of it in "inevitable" fashion. Thus, his solution is that God is free to create any one of an infinite number of possible worlds, but that once one such a world has been created, it develops with a rigid necessity that excludes any possibility of contingency.

"There is one possible Adam", he says, "whose posterity is such and such and an infinity of others whose posterity would be different; is it not the case that these possible Adams (if I may so speak of them) are different from one another, and that God has chosen only one of them who is exactly our Adam?" It is clear from this, in the first place, that this reduces contingency simply to pure inexplicability or arbitrariness, since we are not able to explain why God should have chosen to create the Adam he did, and so there is a point where determination fails or necessity ceases to operate. But the main point here is the one already touched upon, that we seem quite clearly to be able to say intelligibly that Adam need not have had the posterity he did, and it is quite certain that we do not mean that God was free to create a different Adam with a different posterity contained in his "notion". What we do mean, of course, is that we can think of the actual Adam existing under different conditions, being acted upon by different forces. Hence, it would seem that one condition of our being able to recognise contingency is that we observe that the same thing may exist in different environments. What happens to a thing, to put it very broadly, is contingent because the relations into which a thing enters "make a difference to it", but we should never be able to make this assertion unless we could distinguish the thing from its relations and recognise it as recurring in various situations. But the identity theory of necessity, the theory of "notions" and what is involved in them makes it impossible to take any account of this point. For if Adam's having a certain posterity is contained in his "notion" it would be impossible for us to admit that he is affected by what he encounters, or even to speak of him as entering into a variety of situations, or to give an intelligible account of the statement that "Adam need not have had such a posterity". We could not say that anything that actually happened to him was contingent upon any particular circumstance that he chanced to encounter. But it is precisely because a subject has its predicates under certain conditions only, and not under all conditions that we are in a position to say of anything that it need not have been the case. Thus it should be evident that a theory of stages as unfolding with absolute necessity is inconsistent with our empirical recognition of contingency, and the position is the same whether it is said, as by Leibniz, that each successive phase is contained in *each* individual notion, or, as by Hegel, that all development is implicit in *the* notion.

We should need, then, for an account of contingency as an actual relation between finite events, to insist upon the reality of interaction; and that the real connection is between necessitation and contingency (since being necessary for and being contingent upon are reciprocal relations), and not, as Leibniz imagines, between contingency and possibility. If this is the only possible world, he argues, then it is necessary and there is no contingency at all. But this, apart from the whole metaphysical question of possible worlds, reverses the actual order of our thinking; since it is only our recognition of what is contingent upon what that enables us to speak of possibilities. The important point here is that we can assert the possibility of any future occurrence (unless our assertion is guesswork or mere dogma), only on the basis of something we actually see to be the case; thus, as regards any event under consideration, we can discover what conditions are necessary and sufficient for its occurrence, and if they were all present we should not say that the event in question was possible, but that its occurrence was inevitable. But if we find that the conditions actually existing are neither sufficient to produce the given event nor sufficient to ensure its non-occurrence, then we say that its occurrence is possible; so that unless we knew upon what conditions an event is contingent, what conditions are necessary for its occurrence or non-occurence we should not be in a position to pronounce upon its possibility.

This question of possibility is connected with another formulation which Leibniz gives to the distinction between necessity and contingency. He distinguishes "what is true in all possible worlds" and "what is true only in this world", and it is the former which is alleged to exhibit absolute necessity; the latter is, of course, contingent upon God's free choice to create. We do not wish here to consider this distinction but merely to point out that it does at least suggest a division that must be recognised in any logical theory. What is true in "all possible worlds", it might be suggested, is just the sort of thing the science of logic is concerned to study—the general conditions of existence, whatever they might turn out to be. Any logician is constantly asserting that there are certain general conditions to which particular things must conform; we say, for example, that no matter what characters an event may have, to what other events it may be related, nevertheless it will exhibit certain categorical features, such as spatio-temporality. We have, therefore, the initial conception of what is logically necessary (taking logic, as has been said, as the science of the conditions of existence) as distinguished from what is not logically necessary; by the phrase "logically necessary" we mean no more than that there are certain things which logic finds to be the case. Thus, we should say that it is logically necessary that an event B has a cause, but there is nothing in logic, nothing in the assertion that every event is caused to show us that the cause of B is A. In this way, it is possible for us to recognise the "indifference" of particular connections between things to logic; we cannot develop from the science of logic the particular truths of the special sciences, and this provides us with an important sense in which we speak of the contingency of the laws of nature. We can say intelligently that A need not have caused B, that it is possible for A not to be the cause of B, in the sense that there is nothing in logic to show either that the cause of B is A or that it is not A, though logic might tell us that B *must* have had some cause. As regards any law of nature, any true proposition, its contradictory is logically possible, and we might say of the propositions of the special sciences that not being logically necessary, they are contingent.

Along these lines it might be suggested that Leibniz was at least hinting at the truth when he located necessity in what was true of all possible worlds, though, rejecting the theory of worlds we should prefer to say that what we mean by the logically necessary is that which is true of any possible particular thing. The contingent, in the same way, will be that which is not true under all possible conditions, but which holds within certain definite conditions only. This last point suggests the connection between the account of contingency just given and what was said earlier about interaction. Taking logic as specifying the universal conditions of existence, to say that any fact is not implied by logic and that its opposite is not logically excluded is equivalent to the statement that it would not occur under all conditions, but that certain conditions are logically possible in which its opposite would occur. Hence, in this sense, if we say that the fact that "all men are mortal" is contingent, we are saying, in the first instance, that it is not logically necessary or involved in the general conditions of existence; and thus, in the second place, that it occurs only under certain determinate conditions, or that it is contingent upon the truth of certain other propositions which are conditioned in exactly the same way.

If this be the general nature of contingency, there is nothing to suggest that in recognising the contingency of the laws of nature we are casting any doubt upon their absolute truth or their "necessity" in another sense. Propositions are none the less true if there are other propositions that are always true when they are; and so far from contingency being opposed to necessity, we maintain that without necessitation we cannot speak of contingency; unless it is the case that there are conditions necessary for a given event or unless propositions of the form all X are Y are true (where X would be contingent upon Y, or Y necessary for X), we could not speak of contingency at all. What is denied is that causal laws for example are necessary in the sense that they are logically necessary, that their contradictories are self-contradictory or are to be rejected because they are in some other way in conflict with logic. Thus, to repeat, the most general feature of contingency is found in the fact that the characters and connections of events are simply "given", there being nothing in the logical description of being or its conditions to necessitate the facts that our experience presents.

It remains now to develop this account of contingency in somewhat greater detail and especially to show how it is distinguished from a monistic theory which denies contingency by holding that each truth is somehow necessarily involved in reality as a whole. One objection that might be brought against what has been advanced is that the admission of contingency is purely verbal and of no real significance; that the course of events, even on a pluralistic view is absolutely necessitated, even though we may not say that each event is somehow logically involved in the general character of Reality. Or the objection might be expressed in this way. It has been said that events are contingent because, for one thing, either their occurrence or their non-occurrence is logically possible in that neither is necessitated by the general conditions of existence. But, on the other hand, it would have to be admitted that nothing is ever "merely possible" that, while a given event may not have been necessitated by any logical considerations, it must have been necessitated by something, its opposite must have been necessarily excluded. So, it may be maintained, since contingency has so far been described in terms of this negative sort of relation, we might in the end be compelled to admit that there is nothing which is "merely contingent", and that what has been said to be *logically* possible must at the same time be *actually* necessary (if the conditions necessary and sufficient for it exist), or actually impossible (if the conditions necessary for it are absent); and thus that whatever occurs does so inevitably. Now we need not discuss, but straightway admit, that whatever occurs is necessitated by some condition of affairs that precedes, or that if an event has occurred, it was actually impossible, under the circumstances, that it should not have happened. But the main line of reply to the objection has already been suggested, that there is no single necessitating principle for all things, nor even, as Leibniz holds, a multiplicity of original necessitating factors from which everything subsequent unconditionally follows. It will be our business in what follows to bring out the implications of this remark.

One or two traditional ways of taking up the question may first of all be disposed of. One of Leibniz's ways of saying that contingent propositions or matters of fact are in the end absolutely necessary was to assert that the future states of an individual notion are transparent to omniscience, though not transparent to us. This way of speaking, of course, is on a level with the views of those who treat the question of necessity as one of predictability. It is obvious, however, that whether or not we can predict an event is determined by conditions which have nothing to do with the logical character of the event. The important point is that talk about "omniscience" begs the question and slurs over the logical issue. For it obscures from the beginning the conception of *novelty,* the fact, namely, that we cannot infer from our first acquaintance with an event what its effect will be, since there is nothing in the characters of the cause to show what its effect will be or even that it will have an effect. The point will be, then, not whether the course of events would be predictable by "omniscience", but that the conception of "omniscience" is logically confused. Our intelligences are "limited" or "finite" precisely because causes do not contain their effects within them, nor subjects their predicates; and to say, therefore, that the future would be transparent to "omniscience" (if, indeed, the statement is to be taken as any better than an identity) would really be to contend that things do not have the logical characters they appear to us to have, though, at the same time, we could go on to speculate what features they would present to "omniscience" only by arguing from the features they present to us.

Confusions of the same sort appear in the question, "whether the future is completely determined". "That the future is in some degree undetermined", a writer on politics informs us, "is the ineradicable assumption of all thinking. . . When Engels says that 'freedom does not consist in fancied independence of the laws of nature, but in the knowledge of those laws and the possibility thereby created of deliberately making them function towards defined ends', he shows either a naive innocence of the problem he is talking about, or that Hegelian sophistication which is a mockery of innocence. For the fact that my thinking, which assumes to define ends in the future operates with causal laws which assume that the future is already defined is exactly the essence of the problem. The most advanced science, like the most simple practical sagacity insists upon the validity of both these assumptions. . . . These two assumptions are not absolutely contradictory, and if there is a profound mental difficulty in reconciling them, it is in the spirit of science . . . to leave that difficulty standing until a real solution is found". Now, we notice throughout this passage the assumption of absolute necessity, of a pre-established course of development which unrolls under all possible conditions, which we have already had occasion to criticise. As one instance of this, we may draw attention to the question-begging phrase *the* future, as if future events formed one single self-enclosed series, or one single system of events. There is, in fact, no such thing as the future, but many independent futures, and if we remember this, we are not so likely to fall into unreal problems about the inevitability of future events. But what is more to the point here is that we can quite obviously admit that future events are now fully determined without concluding that our purposes cannot affect them. No one would be so foolish as to assert that all natural processes will develop in the same way irrespective of what other processes they encounter, and it is certain that some natural events will be affected by, and hence that their future will be contingent upon, the human purposes with which they happen to collide.

Hence, to the question whether the future is completely determined, we must reply with a further question: By what? Those who, like the writer quoted, are mainly concerned with human freedom really mean to ask whether all future events are determined by present or future events other than human purposes, and unless we believe on other grounds that the mental cannot act upon the non-mental, there is no reason in logic why minds should not affect the future of any non-mental process. But the popular theories of mechanistic determination, where the effect is supposed to be precisely equivalent to the cause, in some sense, and the closely-related doctrine of self-enclosed causal chains, by setting up a false theory of necessity, and by ignoring interaction and, hence, contingency make such a problem of the relation of purposes to the non-mental quite insoluble. It could even be shown that much that has been written on mental "freedom" and "initiative" has been inspired by such notions of necessitation; but without pursuing these questions it is sufficient here to point out that what the contingency of events implies is that there are independent processes which may collide, so that no event is necessitated from the beginning by some single, unconditioned entity.

Now if we go on to seek the logical features of things essential to the sort of contingency we have just described, we may choose, as the most important for our purpose, the fact that events are in space and time. In the first place we may note that processes of various sorts occupy definite expanses of space and require definite durations of time for their working out. Now it is important to observe that the different extents of space and time which different processes occupy are an important factor contributing to their "unpredictability". What will occur in the future is contingent upon what processes a given process will encounter during its history, and what processes it will encounter will depend partly upon what we may call its spatio-temporal "habits". This is a factor that has to be taken into account in the discussion of accident. A preliminary description of the nature of accident would be to say that it is the unexpected, and this might mean that an accident occurs where we are anticipating the working out of a certain process or system in a certain way, when it collides in space and time with another system of which we had not taken account. In general, it might be said that we are not usually so much interested in the time which a given process requires to reach a certain phase as we are in the question of what its phases are; and when this is the case, we are less in a position to anticipate the coming into contact and interaction of systems or cycles hitherto spatially separated.

We do not insist upon this as a final account of accident, but on the main fact underlying it. It is that the space or time that a process occupies is something that is "simply given"; no amount of information about the characters of a thing could enable us to infer how much space it occupies, and however much we might know of the qualities of a certain cyclical process, we could not guess what period of time the cycle would require for its completion. Nor, on the other side, could we say that there is anything in the nature of space-time that would enable us to say that a particular process should require just so much of it. Of course, it is not denied that we may be able to infer how long a process will take to reach a certain stage from information *of the same sort;* thus, e.g., in medicine, it may be possible to argue that a certain affection will reach its climax in a certain time because of its affinity to affections whose temporal "habits" have been observed. But while such uniformities are observable, when a certain phase of one process collides with a certain phase of another, the factors operating on either side are independent: there is no *single* factor to explain why each process should have reached just that phase at any time. So, in the end, we are compelled to say that it "just happens" that independently developing processes should have interacted as they did at a given place and time. On the other hand, we should say that any theory which insists upon the unconditioned necessity of whatever happens must be able to show that there is a single factor or principle determining the spatio-temporal "habits" of the infinite multiplicity of interacting events. And if what has been said of the nature of the spatio-temporality of events is correct, we should expect to find that any such attempt will collapse.

It will be seen, then, that we cannot ignore spatio-temporality in discussing the necessity or contingency of events since it is often upon the spatio-temporal "habits" of things that the occurrence of any event depends. It would, however, give an entirely false impression of the position if it were imagined that the spatio-temporal "habits" of things were in a logically peculiar position. What has been said would apply equally to the causal connections of things, for example; in fact this is only a more detailed exposition of the point that the contingency of events is to be accounted for, fundamentally, by the "indifference" of particular facts to the conditions of existence which characterise them. It may, however, be added that the "mere givenness" of the spatio-temporal "habits" of things is the fundamental condition of the contingency of causal, or any other sort of assertion: this will prove to be most clearly the case in regard to causality.

It may illuminate the argument if we return at this stage to a further consideration of the conception of accident. From what has been said it will be apparent that it may in some sense be maintained that whatever occurs is accidental. Some uses of the term are, of course, legalistic and of no philosophic importance, as, for instance, in the contrast between something done accidentally and something due to negligence, though at the same time a scrutiny of these popular usages would throw some light upon the logical issues. Thus, as regards the contrast often drawn between the accidental and the deliberate, this would seem to be explicable according to the account of accident already given where we have the intrusion into a system of activities of a process of which we had taken no account, as when reaching for my pipe, I upset the ink. Here the accidentally occurring is that which cannot be entirely accounted for in terms of my purposes; what actually occurred was a collision, or intersection, of two independent lines of development. Then, if we take an opposite example, we find that the same sort of explanation would hold. We might be told, for instance, that it is no accident that Trotsky should have opposed Lenin in 1903. To bring out the point of such a statement some expansion is required. To say that it was not an accident that X occurred (the date, in our example, is not the point at issue) is, as it stands, of no significance since events can occur in one way only, by following from necessary and sufficient conditions—though it might be argued that a monistic theory of things, putting stress on the rationality of the "world" could explain contingency so far as it admits such a thing only as that which somehow has no sufficient condition. But, to revert to our example, we are really invited to make a comparison, and what it is intended to assert is that it is no accident that the Trotsky who, in 1938, is following a certain policy would have taken the attitude he did in 1903; that is, what is asserted is that it is not an accident that there should be similarities between the attitudes taken up at the two different periods. And it is not an accident, it is suggested, because it was the same factor which, working in one field in 1903, in another in 1938, produced these similar results. Generally, then, we may say that the similarity between two events, or the intersection of two events, is not accidental if the same factors operating in different fields have produced or determined each of them. Taking another example of something that *is* an accident, we may consider the case of a man injured by a brick falling upon him from a roof. Excluding the possibility that all that might be meant by calling this accidental is that the brick was not deliberately thrown at him, what would constitute the accident would be that the factors determining the brick's being at a certain place at a certain time, on the one side, and those determining the man's spatio-temporal position, on the other, have no connection; the similarity in the two events is not explicable by a common factor operating in each field.

It is not contended, of course, that all uses of the term could be shown to be of this logical character; the most frequent usage would seem to be to indicate simply what was not deliberate or intended, yet even in this usage, as we saw, what is essential to the accidental character of the outcome is that what occurs can not be satisfactorily accounted for by the factors operating upon the one side (our purposes), but that we have to take into account also the independent factors at play on the other. Now what is the most important point to be stressed here is that while we commonly distinguish between the two sorts of cases as, e.g., when *I bring* two objects together (the same factor determining the final position of each), as contrasted with their coming together accidentally (where it is not the same factor operating on both sides), there is really no important logical difference between the two cases. The main point is that we can never find any case of interaction where there are *no* distinct and independent factors operating on each side, so that we may say that every product of interaction is, in the sense illustrated, accidental. Even where our purposes or intentions work themselves out without any intrusion of the "accidental", they are constantly adjusting themselves to (i.e., being affected by) the material with which they are dealing and which they have done nothing to create. This is the issue between those who admit contingency and those who would reject it; the latter would contend that every interacting process is, in the end, determined by a single factor.

Thus, returning to the problem from which this discussion began, we may retain the contention that whatever occurs is necessitated in the sense that any event is brought about by necessary and sufficient conditions operating in a field. But there is no single necessitating principle of all things, nothing which could enable us to make predictions without taking into account the whole multiplicity of events or processes. Of course, in our ordinary way of speaking and of raising questions, the main logical issues are often obscured. For when in practical affairs, we raise the question whether anything is necessary or inevitable—i.e., will definitely happen—we mean to ask whether there are in existence conditions from which the event will follow under any circumstances. But it is to be noted that this procedure obscures the real position, for we do not stress the fact that it is necessary or will happen, only when X appears in a certain field, because in speaking of the necessity of Y, the existence of X in the field has presumably been already recognised and is taken for granted. But when it is seen that before an event is necessary or is necessitated, we must have an interaction of processes, the presence of a sufficient condition in a given field, the aspect of contingency comes more prominently to light. Thus, if it is said that a recognition of the necessitation of things precludes anything but a verbal concession to contingency, it will be seen that this statement appears plausible only because necessity is taken from the beginning as excluding contingency. We should reject the doctrine of the inevitability of what occurs, if by that is meant that what occurs would do so under any conditions, or that there is a single "reason" why all things fall out as they do. What contingency involves, then, is a pluralistic logic, a recognition of the multiplicity of independent interacting processes.

This throws some light upon the common rejection by monistic theories of causal explanation on the ground of its "insufficiency"—what is rejected, in fact, is contingency. Not only is there no single reason why an event should have a certain effect, let alone why every event should have the effect that it has; but in the same way there is no "sufficient" (i.e., single) reason why any two processes, working themselves out in their own way should have intersected at a certain place and time. But in reply to this it may be pointed out that what constitutes a sufficient or insufficient explanation could be determined only from a consideration of the nature of explanation generally; and we could discover what are the logical characteristics of an explanation only by considering the general logical features of the relations between events. Hence, in considering the logical features of things in order to produce a theory of explanation we should discover their contingency and our conception of what is sufficient explanation must be determined for us by the logical features we discover, and though, in idealistic views, causal explanation is declared to be "insufficient" because of the contingency that is involved, it is not difficult to show that the attempt to expel contingency is not very successful.

We may illustrate these points by a brief consideration of some arguments advanced by Bradley and A. E. Taylor. Argument is made difficult, however, by the fact that we are confronted all the time by the dogmatic assertion that such a position as we have outlined cannot be true, even though Bradley admits that he is finally unable to show that it is false. "Why this thing is here and not there, what the connexion is, in the end, between spatial position and the quality that holds it and is determined by it, remains unknown. . . . But any such irrationality and externality cannot be the last truth about things. Somewhere, there must be a reason why this and that appear together" (*Appearance and Reality,* 2nd. ed., p. 577). Yet, although no finality can be achieved in argument of this sort (Bradley's constant recourse to the criterion of "what thought demands" means that the only reason he need give for the rejection of any proposition is that he rejects it), nevertheless, it is possible to construct a sufficiently cogent argument from the admissions Bradley is prepared to make. In the passage quoted, for example, there is really an admission of the position we have taken up. If things "appear together" and if togetherness is somehow an internal relation, then appearance would present no special problems; hence the admission of appearance as outside "Reality" is an admission of the externality, at any rate, of appearing together. Now, in saying that things appear together, it may be meant either that things *occur* together, that the relation of togetherness occurs; or that things seem to be together but are not. Obviously, unless "appearing to be together" means "occurring together", there is no real diversity in the absolute; on the other hand if that is what is meant, there would seem to be no solution to the externality of togetherness.

The same sort of equivocation is apparent in what A. E. Taylor has to say of causality and the reduction of it to the category of ground and consequent. The continuity, persistence and change of temporal events, we are told, lead to contradictions. But, "it is evident that in proportion as our knowledge of any thing or system approaches this insight into the laws of its structure, the processes of change acquire a new character for us . . . they become the self-evident expression of the identity which is their underlying principle. Change, once reduced to law, and apprehended as the embodiment in succession of a principle we understand, is no longer change as an unintelligible mystery" (*Elements of Metaphysics,* p. 163). The artful phrase here is "embodiment in succession". If this means the successive embodiments of the principle, then the principle is recurring in time and the puzzles of continuity and persistence have not in the least been avoided. It is not explained how anything can "appear in succession" without being successive, or how anything can be "embodied in succession" without being persistent through successive phases. We would maintain that the theory of time as appearance could only be stated if "appearing in succession" meant "successively appearing" (i.e., occurring), though what these writers really mean is that reality does *not* successively appear.

What we find, therefore, in these views is the concealed admission of the occurrence or reality of certain relations, and once this admission is brought to light, it can be shown that it is fatal to the rest of the argument. We cannot treat space and time simply as being of no importance, a vagary of the absolute, since, as we have seen, the spatio-temporal "habits" of things are the crux of the question of contingency. Taylor proceeds with his criticism of causal explanation in this way: "The logical defects of the concept need not impair its practical usefulness. Though it can never be ultimately true that any event is absolutely determined by antecedent events, the assumption may be sufficiently near the truth to yield useful deductions as to the course of occurrences, precisely as a mathematical approximation to the value of a surd quantity may, without being the exact truth, be close enough for practical use" (*Elements,* p. 168). The illustration is remarkable. Hegelians, it seems to me, do not assert that causal laws are false in this sense; do not deny, that is, that it is precisely B to which A is related when we say that A causes B. Otherwise nothing could be made of the admission of the complete truth of scientific laws *on their own level,* or within their own sphere. What is denied is, not that there is some relation between A and B, but that this is ultimately the relation of causation. It is obvious that the questions of the "scientific" truth and the "metaphysical" truth of such an assertion could not be separated. For the assertion that A causes B maintains, amongst other things, that A precedes B, that B occurs only after A has occurred, and if this successiveness is left out of account the assertion is meaningless. In reducing causal laws to the axiom of ground and consequent the essential feature of causality is simply ignored, this being all the more indefensible since it is the spatio-temporal relations of events which are one essential condition of that contingency which the category of ground is supposed to overcome.

In attempting to rationalise "the world", Bradley and Taylor are not entitled to put space and time aside in off-handed fashion (to say, for example, that causality is the category of ground and consequent manifesting itself in time), because, as has been emphasised, the spatio-temporal features of things are those upon which their contingency largely rests. Until a monistic theory can satisfactorily show that the various spatio-temporal "habits" of events are necessitated by their single explanatory principle we should not be prepared to accept any doctrine of inevitability, or of absolute necessity which excludes contingency. Taylor says that we have no more reason for holding that an event is determined by antecedent events rather than by events in the future. Certainly, if A is the cause of B, we may admit that B determines A in the same way as A determines B; the formal relation is symmetrical so that we have no A without B and no B without A. But we also say that A precedes and passes into B, and unless Taylor could show that the temporal relations or "habits" of the two events (and of all other events) are determined by the single teleological explanatory principle he is defending, we are not likely to accept the doctrine of a single source of necessity. It could be argued, in general, that all things could not be determined by some "end" for the same reason that they could not be determined by some single factor subdating them. In any case, in affirming that the determination of the present by the past is "insufficient" explanation, these thinkers do not show that determination of it by the future, or by the non-temporal, if such a thing can be conceived, is in any better position. On the contrary, it is admitted that it would not be. For it is precisely the spatio-temporal contingency of events which leads them to reject determination by "the past", and it is precisely the same factor which they admit as a "perhaps insoluble problem" on a theory of the singleness and rationality of all things. "We must, of course, observe", Taylor concedes, "that the principle [of Sufficient Reason] does nothing to solve the perhaps insoluble problem *why* succession in time should be a feature of appearance" (*Elements of Metaphysics,* p. 164). We are left, then, in the curious position that the same contingency or "insufficiency" which, Taylor argues at length, is absolutely fatal to a pluralistic logic is, for a monistic theory, a minor problem to be noted in passing.

We may, as a final point for discussion, return to a passage quoted on a previous page (p. 9), and consider how this account of necessity and contingency bears upon the question of freedom. What seems the most obvious point is that there has been nothing to suggest any conflict between our freedom and the necessary determination of events, but that, on the contrary, interaction or determination is a necessary condition of our freedom. It is, in fact, because the future is not contained *unconditionally* in the past or present, or, as we may rather express it, because the future is not determined independently of what happens to determine it, that it is possible for our demands to act upon things, and to affect their development. The main point, then, is that once the contingency of events is admitted, once the doctrine of mechanistic determination (by which is meant the comprehension of the effect by the cause) is abandoned, our freedom—as the matter of our realising our demands—presents no special logical problem. We find, as we should expect, that it is an incorrect theory of natural necessity that has led to the false formulation of the problem of human initiative. "[Science] insists", to quote the same writer, "that there are, on the one hand, an adequate number of general and reliable uniformities of nature and that there is, on the other, a genuine alternative in thought and action—a possibility that 'I may' or 'may not'. . . . If my activity is withdrawn, the whole chain of events will be broken. The only question then is, can I, or can I not, withdraw my activity."

But it is obvious that in practical activities, at any rate, the question, 'May I', is one that does not present itself. At any stage of our activities we find ourselves with certain purposes or demands, and the question with which we are commonly concerned is 'Can I do X?' 'Is it impossible to bring about Y?' We do not usually stop to consider whether it would have been possible for us not to have wanted to do X. And, clearly, in finding the answer to the sort of question we *do* ask, we are not primarily interested in our own mental processes, but in the modes of behaviour of the things upon which we have to work; we are concerned especially with whether the conditions necessary for our demands to produce a certain result are present. In this sense, our freedom means what we are free to do, and this is not a logical problem but a question of the empirical characters of the various things going on about us. The fact that we are determined from without, meaning that our demands at any time are affected by non-mental conditions is not, so far as this account goes, inconsistent with our freedom but is the logical condition of it. Interaction is a limitation of freedom if the latter term is taken to mean, in the Hegelian manner, self-determination, but in that case the only freedom that remains is "necessity conscious of itself". We are none the less determined if we are aware of the factors determining us, and, indeed, it might be advanced as a *reductio ad absurdum* of the Hegelian theory of a single, all-embracing principle of necessitation that it makes it impossible to explain how our "consciousness" (i.e., our motives or desires) can enter, as *new* factors in the spatio-temporal complex of events and affect their outcome. The conclusion of the monistic position is brought out by Hegel in the *Logic*: "If men remembered that whatever happened to them was only an evolution of themselves, and they only bore their own guilt, they would stand free and in everything that came upon them, they would have the consciousness that they suffered no worry. . . . No doubt, there is a great deal of chance in what befalls. But this accidental constituent is founded on human nature. . . . It is their view of necessity, therefore, which is at the root of the content and discontent of men, and which, in that way, determines their destiny itself" (trans. Wallace, Sec. 147). The dogmatism of Hegel's treatment of "chance" (interaction of independent forces) need not be commented on. But on such a view, or holding such a logic, it could not be explained how men could continue to have wishes or continue to make demands upon things. And we know, as a matter of fact, that our wishes can act upon things, and modify the ways in which they act upon us, but if all that happens to us is equally an "evolution" of ourselves, our wishes are of no importance in the situation and the question of their freedom does not arise. Thus, so far as his monism goes, Hegel's critics are correct in contending that his doctrine is a denial of freedom; though he is also successful against them (i.e., the defenders of free-will) in showing that they cannot avoid the total denial of logic, in other words, avoid indeterminism or "the liberty of indifference".

Thus, on a view which recognises interaction and contingency and rejects any doctrine of the unconditional necessitation of what occurs, there is no "opposition" between necessity and freedom, and no call for a special logic of freedom. It is not pretended, of course, that the sort of freedom we have been discussing is the only conception of freedom that might be put forward. But it is worth while to suggest that what the real problem of freedom always amounts to is whether certain specified demands can or cannot, in the actual mental and non-mental circumstances, realize themselves; whether, in a word, we are free to do this or that. So, in considering any specific question of freedom, we should certainly be concerned with the freedom of some particular thing, just as in discussing a specific question of possibility we are concerned with the ways in which certain things, actually present, will develop. Along this line the argument would be that we can no more speak of freedom in the abstract than we can speak of pure or abstract possibility; the one question, like the other, is a matter of enquiring whether the necessary conditions for a certain outcome are present or absent. What, in particular, we should be concerned with would be a process or system of processes of some definite sort, and in raising the question of its freedom, we should be enquiring whether the conditions necessary for its developing in a certain way, or continuing to function in a certain way are actually present. Actually, we use the term freedom only of human activities and demands, and in demanding freedom certain of our activities, on my view, would be demanding the conditions necessary for their persistence or development. It might also be the case that we should speak of freedom only in relation to human activities of a special character. But the discussion of such questions would demand much space. What we have been mainly concerned to point out is that the "logic" of freedom is simply the logic of interaction and contingency.

THE ENTRANCE EXAMINATION POLICY OF THE UNIVERSITY OF NEW ZEALAND.

I.

By W. Anderson.

Summum ius summa iniuria.

My business in what follows is to examine certain recent applications, within the administrative system of the University of New Zealand, of what have come to be known as the methods of Scientific Pedagogy. It was not to be expected that the exponents of these methods would for long remain content with the restriction of their field of operations to the education of children. Not only will it be universally admitted that these authorities know as much about the work of universities as they do about the proper functions of primary and secondary schools, but above all it must be remembered that the claim of the contemporary "educationalist" is nothing more modest than that he is the repository of a new science which stands to all previous forms of the theory and practice of education, at whatever grade, in the relation which chemistry bears to alchemy or astronomy to astrology.

It will readily, then, be realised that the claim to educational hegemony, hitherto conceded to the university as the custodian of the primary sources of knowledge, is going to be aggressively challenged by this new arrival. In the latter-day professor of "scientific pedagogy" we have a new pretender to the title and authority of "master of them that know"; a title now invested with a grim significance of which assuredly the great Dante had not dreamt when he fashioned and bestowed that august appellation. For a precedent we must go back further to the claims of the Sophists of ancient

Athens, whose ambition it was to set the foot of "knowing how . . . ." upon the neck of "knowing that . . . ."

In 1931 the Senate of the University of New Zealand adopted certain proposals for the "statistical" or "scientific" manipulation of the marks awarded in the entrance examinations of the University, and for the holding of an investigation into what New Educators are pleased to call the objective validity of the methods pursued by its examiners.

The authority for the former is to be found in the Senate minutes for that year, p. 51. (Resolution adopting a recommendation by the Academic Board: "That the proposed system of scaling marks be adopted for Entrance and Entrance Scholarship Examinations in cases where the number of candidates exceeds a minimum to be determined by the Executive Committee after consultation with experts."—Academic Board Minutes, 1930, p. 37.) The investigation into examiners' methods is authorised in a resolution to be found on the same page. (Resolution adopting a recommendation of the Academic Board reported in its minutes, 1930, p. 37: "That the proposed test in marking scripts in English, History and Mathematics be carried out at the Entrance Examination in 1931.")

I shall deal with the latter scheme first. Nothing seems to have come of this "experiment"; at least its results have never been reported to the competent authority. It may, however, be of interest to give here the scheme of its procedure, as indicating the general atmosphere in which the entrance examination question has been canvassed.

The proposed procedure is set forth in Academic Board Minutes, 1930, p. 18. Resolution:

> That the following test be applied to the scripts in English, History, and Mathematics at the Entrance Examination of 1931, Professor Anderson dissenting:[1]
>
> (*a*) A certain number (say 20) scripts to be selected at random by the Registrar before they are sent to the examiners.

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[1] Whatever my shortcomings, I must at least disclaim responsibility for the grammar here.

(*b*) The scripts to be copied in writing by clerks in the office, and the copies given new numbers by the Registrar.

(*c*) The whole, with the copies packed in separate packages from the originals (which would be replaced in their proper places), to be sent to the examiners so that the examiners do not know that the copies are anything other than ordinary scripts.

(*d*) The marking of the copies to be compared with the marking of the originals, to show correlation, maximum difference, mean difference.

(*e*) The marking of the originals will, of course, be used for the purposes of determining the published Entrance Examination results.

That such a proposal got past the "academic" body in the government of the University shows at least this, that no false pride in the professorial status, such as to obstruct "the application of scientific method" to academic functions, exists in New Zealand. Any such attitude of self-regard is reserved for the assertion of Citizen Rights of Academic Free Speech. Doubtless the combination of contempt for public opinion with subservience to governmental bureaucracy is a characteristic product of modern scientific training everywhere. Meanwhile I will concede to the promoters of this late lamented scheme that it did avoid an objection that lies against the methods adopted in the recent Hartog "examination of examinations", namely the New Zealand project ensures that the examiners will read and mark both the original and the "control" scripts under the same sense of responsibility for the results. The Londoners believe that the full equivalent of the examiner's present consciousness that the marking is going to decide the public results of the examination, is securable in a mock examination by the simple device of paying the examiners. Money talks. But of course London is a modern realistic university.

The former-mentioned scheme, however—the "scientific adjustment" of the entrance marks—has passed into operation. Doubtless the other proposal sufficed, by way of a threat, to secure the enactment of the scaling system, and, having thus served its real purpose, could be consigned to oblivion. So much for the promoters' interest in "pure research". The scaling system has been called an experiment, but experiment in the nature of the case it cannot be.[1] In submitting it here to criticism I must make mention of the extreme difficulty encountered in extracting from the authorities any information about the scheme such as is necessary for its scientific scrutiny, even by members of the statutory bodies of the university. This is of especial significance in regard to the interests and "social forces" operative in determining university policy—the composition of the Senate, the extremely conciliatory attitude towards outside interests that prevails in the higher academic direction of the institution, and so on. The consequence is that the effect of the examination is now determined, not by the judgment of the examiners, which is what the public expects and pays for, or by any consultation between them, but by the calculations of an official of the (State) Education Department as to what the number of passes should be.

The entrance examinations of the University of New Zealand are two in number, each uniform for the whole Dominion. (1) A pass in the "Entrance Examination" (still universally known as "Matriculation") constitutes the educational qualification for the admission of candidates for degrees. The only alternative mode of entrance is "Provisional Matriculation", allowed for certain degrees, which is now granted at the minimum age of 21 on colourable evidence of adequate scholastic preparation, but subject to confirmation on the successful performance of one year's work for a degree. (2) The "Entrance Scholarship Examination" is used for the award by the University of two groups of scholarships (and its merit lists by the department for the award of bursaries) tenable at any of the constituent colleges of the University—the "University Junior Scholarships" awarded to the first twelve (at present) on the list, and the "University National Scholarships" to the next twenty on the list.

In both of these examinations the papers are set and the scripts marked, according to a system of local rotation, by groups of the professors of the University (i.e., occupants recognised by the University chairs in the constituent colleges) or heads of departments, with such assistants as they may require. The result of the examination is arrived at, in general, by combining the marks received by the candidate in all the subjects in which he has presented himself—in the Entrance Examination a certain general average is required.

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[1] Any more than the notorious "Russian Experiment".

This "matriculation" is formally a non-competitive examination. Materially this statement has to be qualified in at least two ways. Firstly, we have to consider the keen competition there is between heads of secondary schools to show a high annual proportion of the passes. Secondly, the scaling system has the effect of giving the examination the character of a competition for a more or less fixed number of passes to be awarded in each year. From the candidate's point of view (considering him for the moment as something of a rational agent), a pass is to be secured by his achieving the required percentile average over five or six of the subjects prescribed for the examination. This average is 45, subject to the conditions that the mark in no one subject shall fall below 30 per cent., and in English not below 40 per cent.

In his choice of subjects the candidate must include (1) English, (2) Elementary Mathematics, (3) Latin or Greek or French or German, (4) in the absence of an ancient language,[1] one of the "science" subjects (Mechanics, Physics, Chemistry or "Home Science", Botany, Agriculture, (5) the one or two required, as the case may be, to complete the set, to be chosen from the whole list of the possible subjects of examination, but not so as to cause the set chosen to contain more than two "science" subjects. The standard of the examination is defined by the statement at the head of the prescriptions (Calendar, 1937, p. 14) that "The Entrance Examination will represent the reasonable result of four years' training in a Secondary School"—the minute size of the type indicating perhaps the strength of the hope.

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[1] Greek is not taught in the public secondary schools of New Zealand as, owing to modern enlightenment, the Government will not pay for it.

It is, of course, a major source of all these troubles that the public of New Zealand uses the results of this examination to judge of the educational qualifications of the products of the schools for entry into all manner of walks of life apart from university studies. Though I regard this as a good thing in itself, I must record the fact that the schools and university have not resisted the consequent tendency to demand that the schools do the work in three years instead of four. Thus a pupil who wishes to receive an adequate preparation for university studies must continue at school one or two years after passing matriculation. It may be noted here that the official substitution of the title "Entrance" for "Matriculation" as the name of the examination represents what the authorities thought at one time would cure the public of its bad habits in this connection. A certain popular belief about the ostrich is not unrelated to this.

The Entrance Scholarship Examination is, of course, a fully competitive examination. A list is drawn up of the candidates in order of attainment according to their total marks, and the scholarships awarded to the candidates at the top of the list. This examination is commonly taken after two years' post-matriculation work at a secondary school. A candidate may present himself in any number of subjects chosen from among 16 prescribed, provided that the sum of the maximum possible marks assigned to such subjects does not exceed 2,800. If the total marks assigned to the subjects selected by any candidate fall short of the maximum of 2,800, such candidate may nominate an additional subject, and the maximum marks assigned to this subject are then reduced so as to reach together with the marks assigned to the other subjects presented by him the maximum of 2,800. Further restrictions are (1) a limitation in the number of "science" subjects that may be chosen to three at most, (2) the neglect of marks gained by a candidate in any subject in which he fails to reach one-fifth of the maximum attainable in that subject.

The schedule of possible marks is as follows (or was so at the opening of the discussion; Greek was later reduced to 400): Latin 600, Greek, 500, English 600, French 400, German 400, Mathematics 600, History 300, Geography 300, "Each Science" 300.

I.

In considering the application to these two examinations of the scheme of "scientific adjustment", I shall commence with the case of the latter, the examination for scholarships. It was for application to this as a competitive examination that the method was first propounded, and most of the discussions of it in academic governing bodies have turned upon arguments relevant to the special problems of this examination. But, as adopted, the system has been applied also to the "non-competitive" examination for matriculation. For one thing, more and larger interests in the scholastic world were concerned in the manipulation of "Matric." For another, there is the effect of what Minto called "the bias of happy exercise"; the machinery can be put in operation just as "effectively" upon the results of the one examination as upon those of the other. When a complicated process, requiring technical skill, makes a difference, must not that difference be an improvement? Besides, no further knowledge of the subjects of examination is required of the "expert" operators. After all, $x \times 0 = 0$. Furthermore, as "Matric." is a much bigger examination, stronger justification seems to be afforded, when once it has been brought into the net, for making a permanent job for yet another official, thus helping to keep the brains in the country.

But while the application of such an adjustment to the competitive scholarship examination makes certain highly questionable assumptions which we have here to examine, the scheme as applied to matriculation, besides making these, involves others still, which are equally to be questioned. In other words, there is so much less to be said against "scaling" in the scholarship examination than in "Matric." In both, of course, there is a fundamental and disastrous evasion of the problem.

The proponents of the scaling system, then, had found a problem (as have other people) in the way in which a given rank achieved in one subject in a composite examination tends to count for more (or less) in obtaining an award than does the same rank in another subject. In a memorandum, dated 1926, addressed to the Senate by Professor Shelley, this phenomenon is analysed into two factors; variations respectively between the marks awarded in different subjects (1) in average amount, (2) in range or "spread". The latter is declared to be the more important, so far as concerns the subjects taken by the majority of the candidates in the examination for scholarships.

A simple illustration, imaginary on my part, of the "operation" of this "spread", with no difference in average, would be found in the results of an examination in two subjects, with a maximum possible mark of 20, as follows:

Rank	1	2	3	4	5	6	7
Marks awarded—							
English	13	12	11	10	9	8	7
Mathematics	20	16	13	10	7	4	0

Here the marks in English are more "bunched" together; nobody rises so high as in Mathematics and nobody falls so low; in Mathematics it is "easier" to score full marks, and in the same sense "easier" to score none at all. Now in this case, even supposing the candidate who was first in Mathematics to be last in English, he would still have more marks than the first in English, if in Mathematics the latter came anywhere lower than second.

In the actual examination in 1925 the following case is quoted in a memorandum by Professor Shelley supplementary to that above mentioned. Here a candidate came third on the published list. He did not take English (600)—he took Mechanics (300) and Additional Mathematics (300) instead. (These two are small "constituencies", with a high average mark in that particular year, so that here the factor of average is added to that of range or "spread".) This candidate was placed first and second in these two subjects respectively, but had he taken English and been placed first in this 600-mark subject, he would have dropped eight places in the scholarship list. This, Senators are told, shows the need for the "scientific computation of aggregated results". It certainly shows what would be the consequence of applying the formula so denominated.

Before considering the indictment of the former system, it is desirable to look at the process by which the "scientific" result is reached, inasmuch as the assumption of the validity of this process—guaranteed by the "expert" status of its promoters, to be taken on trust by mere scholars and governing laymen—is in fact the solitary ground advanced for the indictment of the then existing system. The nature of the process as then proposed by Professor Shelley, is indicated by the administrative procedure he recommended. In essentials it is in force today. I quote from the memorandum already cited:

> (1) Examiners should be allowed to mark as they think best for themselves without reference to any required average.
>
> (2) Examiners should be allowed to mark on whatever maximum they find most convenient.
>
> (3) The marks received from the examiners should be sent to a standardising committee consisting, say [*sic!*], of a statistician and a professor of Education, who will find equivalents for the marks of each candidate in the various subjects with a standard average and a standard spread, *and according to the discriminative value placed upon each subject by the Schedule of Marks in the Calendar.* (Italics mine for further reference.)
>
> (4) The list of marks should then be sent to the Senate with such remarks upon individual candidates (only known as numbers, of course) as they may think deserve consideration in the award of the scholarships.
>
> (5) The standardising committee should be required to supply with the marks a description of the method they have used in arriving at their result.[1]

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[1] As this worked out in practice it meant that after many years the writer was able to persuade the Academic Board to call for such a statement from the scaling officer, which duly arrived marked "confidential". It was certainly confidential in the sense of amounting to the familiar "expert" demand for a generous vote of confidence.

The standard average and range ("spread") referred to in paragraph (3) are determined by the fixing of upper and lower quartile values, the same for all subjects, with the consequent transformation of the various marks as a whole. The arithmetic of the process is explained in a report by the late Professor D. M. G. Sommerville on Professor Shelley's memoranda, from which I quote:

> The method . . . . consists in fixing two points in the distribution, half-way from the centre (median) to each end (the two "quartiles"), and assigning definite marks, say 60 and 40 respectively, to the two candidates A and B who occupy these places. The marks of the other candidates are altered by the linear transformation which changes the marks of A and B into 60 and 40 respectively.
>
> Thus if x is the original mark of any candidate, x′ the transformed mark,
>
> $$x' = px + q,$$
>
> where p and q are certain fixed numbers easily found by substituting $x = A$, $x' = 60$; $x = B$, $x' = 40$. The final formula is
>
> $$x' = \frac{20}{A-B}\,(x + 2A-3B).$$
>
> The reduction is therefore equivalent to adding first the fixed number 2A–3B and then changing the marks in the ratio 20 to A–B.

Professor Sommerville, with reference to certain *prima facie* objections to the process, went on to add the following comments:

> *If the distribution of the candidates is fairly symmetrical*, the medians and means in the different subjects will nearly correspond. As the method is not based on the value of the mean in the presence of a few very poor candidates or a few exceptionally good ones, the good ones will not suffer by the presence of the poor ones, or *vice versa*. If the marking is all too low, *but with fairly normal distribution*, the marks will be raised nearly proportionally; similarly if the marks are proportionately generous.
>
> When the number of candidates is large the distribution will usually be fairly normal, and the marks to be added or subtracted before the proportional raising will not be very large. With small numbers, however, it may happen, *when the candidates are bunched in the middle*, that a few low candidates may have their marks reduced below zero; or on the contrary if they are *too much spread in the middle*, that some of the high candidates have their marks increased beyond 100. (Italics mine in all cases.)

The passages I have italicised make it clear that the departures from "normality" contemplated by Professor Sommerville, *viz.,* a more acute curve and a flattened curve, do not entail a departure from symmetry. In the case of the Matriculation Examination, on the other hand, the matter of skewedness in the curve becomes the important "abnormality", entailing as it does the questions of the adequacy of the standard of work in the schools, and of the legitimacy of different types of examination. In the case of the scholarship examination, however, we notice that according to Professor Sommerville's analysis the system of scaling based on the fixing of the upper and lower quartiles is liable to fail precisely on the point on which it was advocated as a cure, namely, the possibility of an undue influence of the subjects taken by small numbers of candidates. What the methods of examining and marking are which "produce" bunching or spreading in the middle (or, in the case of Matriculation, bunching at the bottom), and indeed whether or not these effects are to be regarded as counts against the examination system at all, are questions I shall deal with later. Meanwhile the fact remains that in so far as these effects are counteracted under the present *régime,* it means that the officials have quite unscrupulously interpreted an instruction to scale the marks according to a certain system as a grant of *carte blanche* to scale them in any way they like, *i.e.,* in whatever way is found necessary to conciliate scholastic vested interests.

Coming now to examine the case for the change, we find a remarkable state of affairs. The case, we discover, turns wholly upon an allegation as to the original intention embodied in the allotment of varying possible totals to different subjects respectively in the examination schedule. The scheme is given a specious air of being the scientific procedure by which the then existing system could be made consistent with its own specified conditions. The whole case for the change is found to depend on the propriety of the phraseology of paragraph (3) in Professor Shelley's memorandum given above: "the discriminative value placed upon each subject by the Schedule of Marks in the Calendar."

The essential sophism is contained in the following statement taken from the same memorandum:

> I assume that the object of giving a maximum of 600 marks to Latin, English, and Mathematics, 500 to Greek, 400 to French, 300 to the other subjects, and 200 to Geography, is to ensure that Latin, English and Mathematics shall have equal value in determining a candidate's position in the Scholarship list; also that these three subjects shall each have twice the value of such subjects as History, Chemistry, Additional Mathematics in determining the final order.

This statement can mean nothing else than that the whole intention in allotting these maxima to the various subjects was to ensure, *e.g.,* (1) that a candidate's achieved rank in Latin, English, Mathematics should have equal value the one with the other in determining his position on the scholarship list, and (2) that his achieved position in any of these subjects should have in each case twice the value of his rank in any of such subjects as History, Chemistry, Additional Mathematics in fixing his place on that list. A prospective influence of possibilities on the will and intellect of the candidate is identified with the influence of an achieved result upon a further result.

Now when an orator succeeds in producing an identification of this sort in the minds of his auditors, they are prepared to be duly impressed by such "glaring instances" as that got from Professor Shelley's analysis of the returns for 1925, purporting to demonstrate the "influence" of varying range ("spread") in the marks actually awarded by the different examiners. If in that year, we are told, the marks in English had been discarded altogether, the "spread" in English was so small that four only out of 32 scholarship-holders would have changed places with non-scholarship-holders. Yet English is a 600-mark subject. How dreadful! All that now remains to be done in order to "prove" the inconsistency and failure of the former system is to draw up a "deadly parallel" between the "intended" determining values of the various subjects and their "actual" determining values. Here it is:

	Determining Value		
	Intended.		Actual.
Latin	3	..	3
English	3	..	2
Mathematics	3	..	4
French	2	..	1½
History	1½	..	1¼
Chemistry	1½	..	2½
Magnetism and Electricity	1½	..	3
Heat	1½	..	2¼
Botany	1½	..	1¼
Home Science	1½	..	1¾
Agriculture	1½	..	1¼
Additional Mathematics	1½	..	4
Mechanics	1½	..	2¼
Geography	1	..	½

This, in the supplementary memorandum referred to above, is followed by a table, giving in parallel columns the comparison of candidates' positions in the published list in 1925 with their positions and marks "after scientific adjustment". A portion of it is now given:

"Junior Scholars."

Published Position.	Scientific Position.	Scientific Mark.	Published Position.	Scientific Position.	Scientific Mark.
1	1	1980	23	25	1575
2	3	1857	24	24	1580
3	9	1746	25	27	1560
4	10	1733	26	29	1551
5	2	1882	27	28	1558
6	6	1774	28	37	1511
7	16	1665	29	44	1463
8	5	1793	30	30	1547
9	15	1696	31	22	1602
10	4	1841	32	35	1517
11	12	1717	33	38	1507
12	7	1773	34	43	1482
"National Scholars."			35	31½*	1537
13	18	1641	36	34	1518
14	26	1567	37	36	1512
15	13	1710	38	41	1491
16	8	1756	39	45	1445
17	14	1707	40	31½*	1537
18	11	1725	41	39	1503
19	21	1614	42	42	1489
20	17	1649	43	33	1522
21	19	1627	44	40	1493
22	20	1617	45	23*	1583

The three candidates marked with asterisks who failed to win a scholarship come above the scholarship line on the "adjusted" result.

Candidate 45 (published list) advances to position 23, and instead of being 13 places below the scholarship line should "actually" be 10 places above, and thus easily win a scholarship, etc., etc.

What else was there left to Senators to do than to adopt the scaling system? *Nescis, mi fili, quantilla sapientia regitur mundus.*

The only other distinguishable count in the indictment occurs where we are told that the "adjusted" results give a very much smoother curve than the published results, and that this tends to show that the degree of reliability is greater, since "the curve of capacities and achievements of scholarship candidates throughout New Zealand each year is found to approximate closely to a curve of normal distribution". The argument here is on common ground with that for the application of the "scaling" process to the "non-competitive" Matriculation Examination; so we can postpone its consideration until we come to deal with the wider issue.

I submit that the assumption about the intention underlying the assignment of the *maxima* to the various subjects is nonsense. I do not know if the people who made the allotment in its original form are still alive to be interrogated. But various changes have been made in the *maxima* for various particular subjects from time to time, and we do know something about them. Some of them have been made since the scaling process was inaugurated, and if the influence of achieved positions had been the governing consideration, obviously that could have been corrected by scaling without altering the *maxima.* (Indeed, a curious feature of the situation is the way in which the academic bodies of the University have gone on developing the system on its original lines, apparently quite unaware of the difference made to the whole system by the adoption of scaling to which they had committed themselves, as if that belonged to a different world, with "experts" of its own.) Geography, for instance, has since the time of the memoranda, been promoted from 200 to 300, and Greek, which had been, as we saw, reduced from 500 to 400 while the fight was on, has since, I understand, been further reduced from 400 to 300. Will anybody come forward to say that these changes have been made on a technical consideration of the effect of "spread" in the actual marking in these subjects upon the result as a whole? Does anybody believe, then, that the original allotment was based upon expectations as to the relative influence of actually achieved positions in the several subjects on the final position of the candidates?

The obvious considerations determining the selection by the University of subjects for the scholarship examination, and the relative "weights" allotted to them in terms of *maxima* in possible marks, are such things as the educational values of the subjects as a preparation for academic studies, their values as evidence of the existence of qualifications for entry upon such studies, the existing resources of the secondary schools of the country for instruction in the different subjects, and the like. As addressed to the candidate the regulations say that so many total marks are available in this subject and in that; that it is for him to develop and actively use the requisite knowledge accordingly. That the achievement of a given actual position or rank in one subject does not usually take its possessor so far towards a scholarship as the same rank in another is not the main point for him. His problem is to get full marks. Any information he may collect as to the greater difficulty of getting full marks in one subject than in another is for him the problem of surmounting this difficulty. He is, in other words, taken to be a rational agent. (When I say "for the candidate", in all this, one may of course add "for his teachers".) The examination, then, is to be approached as a single problem, in which various topics are to be brought to intelligibility in thought and expression. The *maxima* are something addressed prospectively to the candidate's will and intellect, not retrospectively, in the form of results achieved, to his "nature" (if indeed anything could be said to be "addressed" to that).

In all this I am, of course, not concerned to deny that there is a real problem in connection with the greater "discriminative value" of some subjects than others, or of their handling by some examiners than by others. But I hold that there are ways of interpreting and handling these "anomalies" which do not transgress the conditions of the ordinary rational view of the nature of intellectual work. Whereas the fatal objection to the "scientific" method before us is that it precludes access to the means of any such diagnosis, actively obstructs the consideration of rational remedies for genuine abuses and shortcomings, and makes for educational stagnation and the protection of vested interests.

It will have been evident that, for the advocates of "scientific" adjustment, rank is everything and the numerical mark nothing. While the assignment of a value to the work of each candidate in comparison with what is possible no doubt must come first in the mind and informed judgment of the examiner, the place of this in the system is merely accidental and pragmatic in relation to the arrangement of the candidates which for the examiner is a mere by-product. The examiners are mere instruments, working in darkness to create the light, used by a higher power to ends that do not concern them. The place of knowledge in the educational system is reduced to that of a mere occasion for the display of "abilities" and "achievements." Wherever its deliverances appear to conflict with the calculations of the "expert" in the distribution of abilities and achievements, it must be discredited. Why the serial arrangement of the candidates should be treated as above criticism and the substantive marks beneath criticism remains an enigma of the system.

Differentiation between individuals, however, there must be at all costs. We have seen how the "curve of normal distribution" is taken as mandatory for the results. Associated with this is the contention that this distribution applies to each of a set of unitary "abilities", and again to the related "achievements", corresponding to the several subjects of study in the schools. But here again, why not be thorough? Why reject the approach to the examination with its various subjects as a single intellectual problem, and yet treat as sacrosanct the traditional "subjects" themselves as representing these abilities and their deployment? Once you have accepted the possibility of an evaluation of knowledge from a (biological) standpoint outside knowledge, must you not regard the traditional divisions of knowledge, or "subjects", as mere historical curiosities, embodying merely accidental concatenations of the "real" unitary "abilities"?

Nor is it an answer here to say that you are, as "practical" men, taking up the problem at the point where you find it. How can it be any sort of "step in the right direction" (*viz.*, back to Nature) to adopt a course that conceals rather than reveals the real abilities? What is this but to perpetuate the despised intellectualism and rationalism, making it more secure than ever under a "scientific" guise? The result will be more arbitrary than ever. What the exponents of "scientific adjustment" ought to have done is to justify on their merits their proposals for a uniform configuration for all subjects of the marks awarded; to justify them, if they can, in terms of their own doctrine of atomic "abilities", without recourse to the alleged intentions of pre-scientific examining authorities in assigning possible totals with no such doctrine in view.

For such a justification, however, we look in vain, and that for a very good reason. When the problem has been set in terms of the play of biological "abilities", there is nothing left to guide us in deciding in particular cases whether equal "determining values" should be allotted to the product of different "abilities" or not. To get a decision, some arbitrary standard, such as the existing schedule of *maxima,* has to be found and dragged in, and a meaning read into it which is foreign to its original intention. Your scientific reformer must always fall back upon tradition at some point.

But now it is open to a critic, working within the same naturalistic doctrine of "abilities", to argue that the alleged anomalies of average and range of marks—or some of them—are rooted in the nature of things, in the character of the problems to which the "abilities" are severally addressed. For such a critic they represent, if not Nature's revenge upon the "human" scheme of *maxima* laid down, at least the limits imposed by Nature upon its application.

It is even possible, proceeding further on these lines, to contend that, for example, that superior influence of Mathematics which appeared in column B of the table of "determining values" given above, represents a fact in Nature; that "mathematical ability", at least at this stage in the individual's development, is in fact the most accurately diagnostic of subsequent achievement in all branches of academic study.

I do not myself adopt this basis of criticism, or any part of it. I confine myself here to pointing out that it is one which the advocates of "scaling" are not in a position to rebut. Not only is this so, but their chosen method is incapable of distinguishing between "anomalies" that may be thus due to the nature of the subjects themselves and those that may be attributed to the vagaries of particular examiners. Dogmatically and by violence it treats the former as merely cases of the latter.

(*To be concluded.*)

THE STATUS OF SENSA.[1]

By W. A. Merrylees.

I.

The ordinary man does not speak of 'sensa', but of 'sensations'. Some modern philosophers, however, prefer the term 'sensum', on the ground that 'sensation' is ambiguous.[2] The whole situation to which the ordinary man means to refer when he says he has a sensation, they contend, is analysable into two constituents: (1) an act or state or function of sensing, (2) what is sensed—the object to which the sensing refers. The term 'sensation' could be taken to mean, and in common parlance probably is used to mean, either the former or the latter, or again the whole sensation-situation. To avoid the ambiguity which the use of the term 'sensation' would involve, they prefer to speak of the first constituent as the *sensing,* and of the second as the *sensum* (or sense datum). Assuming that this analysis is correct, the sensum, according to its original definition, is the objective constituent of a sensation-situation. Now we may either restrict the term to this, in which case the sensum will be the objective constituent of an actually occurring sensation-situation—that which is sensed while it is actually being sensed; or we may apply the term to anything which could be sensed or constitute the objective constituent of a sensation-situation, whether or not it is actually being sensed at the moment. The denotation of the term will be the same in both cases, and the two definitions in effect equivalent, if it is the case that that which is sensed can have no existence except while or as being sensed, or, in other words, if the sensum has no existence except as the objective constituent of a sensation-situation. This would be the case if (1) the being of the sensum consisted in being sensed, or (2) the sensing produced and maintained the sensum, the sensum ceasing to exist as soon as the sensing ceased, or (3) the sensum were inevitably sensed whenever it came into existence.

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[1] Based on an address delivered at a joint meeting of the Australasian Association of Psychology and Philosophy and Section J of the Australian and New Zealand Association for the Advancement of Science, Sydney, 22nd August, 1932.

[2] E.g., Prof. G. F. Stout. Cf. "Prof. Alexander's Theory of Sense Perception", *Mind,* October, 1922, p. 386.

The first alternative is inconsistent with the analysis of the sensation-situation into sensum and sensing, for if the being of the sensum consisted in being sensed, the sensing would be the sensum, the two would be indistinguishable. The second alternative is consistent with the analysis, but it may be urged that it is incompatible with the meaning of the term sensing. On this view, if the sensation-situation is to be analysed into an act of producing and the object produced, we are not entitled to call the one sensing and the other the sensum. For sensing is not an act of producing; we can sense only an already or independently existing object. The conception of sensing as an act which produces its own object is a contradiction in terms. This contention is probably justified. But it does not, by itself, justify the conclusion that in the sensation-situation the act does not produce the object. It may instead show that we are not justified in calling the act sensing, at any rate if we wish to attach its normal meaning to the term. If the truth is that the act is not sensing in the normal sense, we have no ground for concluding that the sensing does not produce the sensum. This conclusion cannot be justified by any a priori or phenomenological argument; it must be justified, if at all, by examining instances of sensation-situations. The third alternative reverses the relation between the two constituents; it asserts, not that the sensing causes the sensum, but that whenever a sensum exists it invariably and inevitably gives rise to the act of sensing it. If this assertion is to be substantiated, it must be by the examination and analyses of instances of sensation-situations.

In proceeding to examine typical instances of sensation-situations, I propose to define a sensum as anything which could be the objective constituent of a sensation-situation, but to do so subject to the qualifications that it may be the case that (1) nothing now existing which is not now such an objective-constituent could possibly become one, i.e., that in effect this definition is precisely equivalent to the other one, (2) the analysis of the situation into an act and an object may not be justified, and (3) if it is, the act may not be one of sensing in the normal sense.

1. A TOOTHACHE OR OTHER PAIN.

A toothache, according to Prof. G. F. Stout, is "a sensation in the most popular and in the most technical sense of the term". Stout makes it clear that by a sensation he here means a sensum.[1] Now, if a toothache is a sensum, it must be the objective constituent of a sensation-situation, the other constituent of which is a sensing. And what is true of a toothache is presumably true of any other pain, including pains which can be produced at will, e.g., by pressing against a sharp point or pricking oneself with a pin. The question then is: when I experience toothache or any similar pain, can the sensation-situation be analysed into two constituents, a sensing and a sensum? It can be so analysed of course only if the situation really consists of these two constituents. We have ground for asserting that it does only if either (1) we can by inspection or introspection 'see' that they are distinct, as we can for instance see that two colours are distinct; or (2) we can point to some other fact which could not occur if they were not distinct. Now, in the case of pains which I can produce at will, I not only fail to detect two distinct constituents, but get the impression that they do not consist of two constituents. Though we speak of feeling a pain or a toothache, these do not seem to be entities distinct from our feeling of them as a sensum must be from the sensing of it, but rather modes of feeling. Perhaps we speak of 'feeling a pain' by the same idiom according to which we speak of 'going a walk'.

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[1] Cf. Manual of Psychology, 4th Ed., p. 123: "sensation—or sensa, as they may be called."

Can we then point to any thing, the occurrence of which is possible only because the situation does contain two distinct constituents? Prof. G. E. Moore's argument in regard to the sensation of blue and the sensation of green[1] suggests that such a thing is to be found in the fact that both a headache and a toothache are sensations, but nevertheless are recognisably distinct. This, his argument suggests, is possibly only because each contains two constituents, one of which is common to both, the other of which differs in the two cases; the common constituent being the act of sensing, while the constituent which is different in the two cases is what is sensed—the sensum. The argument, however, is open to objection. If we grant that a toothache and a headache are both sensations only in virtue of the fact that they have something in common, it does not follow that the common factor must be the act of sensing. Their names suggest that what is common is the fact that both are aches, painful modes of feeling. I can see no way of proving in this manner that the analysis of a sensation-situation into two constituents is justified, unless we can show that one constituent can exist in the absence of the other. In the nature of the case a sensing cannot exist without a sensum, but it may be the case that a sensum (or sensible) can exist without being sensed. Hence, to prove that the analysis of a given sensation-situation into a sensing and a sensum is justified, we must show that it contains a sensum which can exist without being sensed. In the present instance we would have to show that we can have a toothache without feeling it. Now, not only have we no reason to think that this is the case, but we have good grounds for maintaining that it cannot be the case. For if I can have a toothache without feeling it, then I am never entitled to say that I have not toothache. For the only ground I can ever have for saying that I have not toothache is the fact that I am not feeling it, and this constitutes a ground for saying I have not toothache only if I can never have toothache without feeling it.[1] If, then, I can have toothache without feeling it, I can never know, when I am not feeling it, whether I have toothache or not. But in that case I can have no ground for asserting that I ever have toothache without feeling it.

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[1] In his Refutation of Idealism, Philosophical Studies, p. 17.

If my argument is sound, I cannot have toothache without feeling it. That, however, does not necessarily imply that the being of the toothache is constituted by the feeling; it may be due simply to the fact that toothache is of such a nature that it is always felt as soon as it comes into being. Consequently the fact that it cannot exist without being felt is not necessarily incompatible with the analysis of the sensation-situation into sensing and sensum. But it does make it impossible to prove that the analysis is legitimate. We must therefore conclude that in the case of toothache and similar sensations it is doubtful whether the analysis of the sensation-situation into sensing and sensum is legitimate. If it is legitimate, then Prof. Stout is undoubtedly right in treating toothache as a sensum, for it certainly is not an act of sensing as opposed to something sensed. But, in any case, it is not the type of sensum in which we are interested.

2. TOUCH.

Reid claims that in the case of touch we must distinguish between the sensation we feel and the quality of the object touched. "Pressing my finger with force against the table, I feel pain, and I feel the table to be hard. The pain is a sensation (experience) of the mind, and there is nothing that resembles it in the table. The hardness is in the table, and there is nothing resembling it in the mind. . . . I touch the table gently with my hand, and I feel it to be smooth, hard and cold. These are qualities of the table perceived by touch; but I perceive them by means of a sensation which indicates them. This sensation not being painful, I commonly give no attention to it. It carries my thought immediately to the thing signified by it, and is itself forgot as if it had never been. But by repeating it, and turning my attention to it, I find it to be merely a sensation, and that it has no similitude to the hardness, smoothness or coldness of the table which are signified by it."[1]

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[1] It may be objected that this argument is invalidated by the not uncommon experience of 'forgetting' an ache or pain when we become absorbed in something else. The fact that we feel it again as soon as we cease to be absorbed shows, it may be urged, that it was there all the time, though, being absorbed, we did not feel it. But is there any reason for denying that the pain itself ceases as we become absorbed, and returns again when we cease to be absorbed? It is presumably reasonable to suppose that the physical disturbance which gives rise to the pain persists throughout. But there seems as good ground for arguing that the fact that we cease to feel it proves that the pain has ceased, as for arguing that the fact that the bodily conditions which give rise to it persist proves that the pain persists. It is no use contending that the pain continues to exist implicitly, for we can also maintain that it is implicitly felt.

Let us first discuss Reid's contention as far as possible in his own terms; we can ask later if or how the distinction of sensing and sensum is applicable. Introspection seems to confirm Reid's contention that, when we touch an object, we both experience a sensation and also become aware of a certain quality of the object. But I cannot accept his account of the relation between the sensation and the quality. He is not, so far as I can see, justified in saying that I perceive the quality "by means of a sensation which indicates" it, or that the sensation "carries my thought immediately to the thing signified by it". Whether we take an instance in which we are normally aware both of the sensation and the quality, or a case in which we do not normally notice the sensation, we are not conscious of any transition from the nature of the sensation to the quality of the object. Take the case of pressing my finger firmly against a pointed object. Here I am normally aware both of the pain and also of the sharpness of the object. But I do not seem to be first aware of the pain, and then from that infer, or otherwise become aware of, the sharpness of the object. I seem to perceive the sharpness directly, at the same time as I feel the pain. Not only do I not feel that I infer the sharpness from the pain, but further, if I concentrate on the pain, I do not seem to be able to infer, or to be prompted to assert, the sharpness of the object.

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[1] Quoted by Prof. Stout, Analytic Psychology, Vol. 1, p. 43.

It seems possible to a certain extent to verify this experimentally. If I press the point of a sharp pencil into my finger, the pain persists after the pencil is withdrawn. But I seem quite unable, by attending to the pain, to determine what caused it, and do not seem to regard it as indicating anything. The position appears to be much the same if I rub my finger across the table. I feel the slight roughness of the table, and can also, I believe, detect a certain sensation which I cannot adequately describe, but which differs from the sensation I experience if I rub my fingers over the cover of the book beside me. But, if I concentrate my attention on the sensation, it does not seem to mean, or signify, or "carry my thoughts to", anything else. It is no doubt true that it is only through sensations that we can become aware of sensory qualities. But it does not follow, and is not the case, that it is through the sensations Reid mentions that we become aware of the qualities he mentions. We become aware of the hardness of the table, to take his first instance, not through the sensation of pain, but through another experience which would ordinarily be called a sensation of hardness. These are two quite distinct sensations, which occur together only because the conditions which give rise to one also give rise to the other. The sensation of pain is a sensation in the same sense as a toothache. If, therefore, it forms a constituent of a sensation-situation which is analysable into a sensing and a sensum, it is a sensum, not an act of sensing.

Turning to the sensation of hardness, one difference between it and the sensation of pain immediately forces itself on our notice. When we speak of a sensation of pain, we do not mean to suggest that the pain is one thing, the sensation another, and that between them there holds the 'relation' which is indicated by 'of' in such phrases as 'my idea of X', 'my notion of X', 'my knowledge of X', 'my impression of X'. We use the phrase simply to indicate the kind of sensation to which we wish to refer. We could equally well, and perhaps more naturally, say ‘a painful sensation’. With a sensation of hardness, on the other hand, this is not the case. We do not speak of a ‘hard sensation’, but always of a ‘sensation of hardness’. We do so because we feel that the sensation is one thing and the hardness another. With the pain it was the sensation which was painful, but in this case it is not the sensation but the table which is hard. I admit that you can distinguish between this sensation and others only by reference to hardness. If, however, you say that the hardness qualifies the sensation no less than the table, you must distinguish two modes of qualification, for it does not qualify the sensation in the same way as it qualifies the table. We may express this difference by saying that it qualifies the table *factually*, but qualifies the sensation *functionally*. For the sensation of hardness means the peculiar awareness of hardness I have when I press my finger against the table. This awareness is functionally qualified by hardness in that it is its function to be aware of hardness.

The sensation-situation which occurs when I have a sensation of hardness, then, is undoubtedly analysable into sensing and sensum, for this peculiar awareness of hardness, which we speak of as the sensation of hardness, is simply the sensing of hardness. The correlative sensum is the hardness of which we are thus aware, that is, the hardness of the table. The analysis is undoubtedly justified; for here we can both see that the sensing and the sensum are distinct, and also, so we ordinarily believe, have ground for maintaining that the sensum can exist in the absence of the sensing. The two are clearly distinct because the sensing is my act or state, the sensum is a quality of the object. Everyone, at any rate when not philosophising, believes that the table is hard whether or not anyone happens to be sensing its hardness. Of course, it is only by feeling it and sensing its hardness that we can tell whether it is hard. But we can at will cease to sense it, or begin to sense it again, simply by withdrawing our finger or touching it again respectively. Whenever we choose to touch it, we sense it as hard. In the case of things which sometimes feel hard and sometimes feel soft, e.g., wax, we have ground for thinking that these changes are due to changes in the object itself, for they correlate with other observable changes, e.g., changes in temperature. We can maintain that the object is hard when I am not feeling it, without being precluded from determining whether it is hard or not. For we can always tell whether the fact that I am not sensing the object as hard is due to the fact that the object is not hard, or to the fact that I am not satisfying the conditions requisite for sensing it, that is, am not touching it. Further, when I am satisfying the conditions, if I do not feel it as hard, I do not merely not feel it as hard, but feel it as soft.

There seems good ground, then, for maintaining that the sensation-situation which occurs when we 'have a sensation of hardness' is analysable into a sensing and a sensum; that the sensation of hardness here means the sensing of the hardness, and that the sensum is the hardness, which is a quality of the object.

We may be told that, though we can perceive that an object is hard, we cannot sense it as hard, and therefore have no right to call the quality of the object a sensum. If we distinguish between sensation and perception in the way in which psychology does, and maintain that our awareness of hardness is sensation only in so far as it is due merely to the stimulation of the sense-organ, then no doubt it is true that we cannot sense, but only perceive, an object as hard. For we cannot experience a mere sensation of hardness; we cannot sense hardness without perceiving something as hard. We cannot even by introspection distinguish within the perception two parts or factors, one of which is, or is due to, present sensation, the other to past experience. The only way in which we can form some conception of what is merely sensed is by considering what features of perception are possible only in virtue of past experience. Now there seems good ground for maintaining that it is only in virtue of past experience that we perceive the hardness as a quality of an object. That, however, does not mean that what is sensed is not a quality of the object; it only means that, if or in so far as we merely sensed it, we would not be aware of it as a quality of the object. It certainly does not mean that it is sensed as something other than a quality of the object, for instance as a state of mind or psychical event. It is not something distinct from perceived hardness; it is not a sort of *tertium quid* which mediates between the act of sensing and the quality of the object. It could be simply sensed only at a stage of experience at which the distinction between substance and attribute had not yet arisen. But though, at that stage, it could not be apprehended *as* a quality of an object, it *is* the quality of the object, and not something else, which is apprehended. This would still be true even if (as in fact is probably the case) our awareness of the hardness (as distinct from our regard of it as a quality of the object) were not, as we have assumed, entirely a matter of sensation, but in part due to past experience. It would still be the quality of the object which we sensed, though our mere sensing of it would not represent the whole of our awareness of it.

The only way in which we could distinguish between the sensum and the quality (and then only if we modified our definition of a sensum) would be to say that what *qua* qualifying an object is a quality, *qua* being sensed is a sensum. Even so, unless we maintain that the function of sensing modifies what is sensed, so as to make it other than it is as qualifying an object, it would still be one and the same thing which from one point of view is a quality, and from another point of view a sensum. If the function of sensing does so modify that which is sensed that this is not true, then I do not see how we can maintain that we perceive the quality of the object. At any rate we could do so only if we held that the other factors involved in perception compensate for the distorting influence of the sensing. It is obviously more reasonable to maintain that the act of sensing does not distort but apprehends its object. But if that is so, what is sensed is identical with what qualifies the object, and so there seems no point in distinguishing between the sensum and the quality.

It may be contended that our whole argument rests on a false assumption, namely, the assumption that the sensation of pain and the sensation of hardness are two distinct sensations. These, so it may be urged, are really the two constituents of a single sensation-situation, the 'sensation of pain' being the function of sensing, while the 'sensation of hardness' is the sensum which it senses. If this were the case, there would, I think, be good ground for regarding the sensum (as some of his statements suggest that Prof. Stout does[1]) as something distinct from the quality, through the sensing of which we come to perceive the quality. For it is obvious that what we call a sensation of hardness is neither a quality of the external object nor identical with the sensation of pain. It is clearly distinct from a quality of the object in that it is mine, whereas the quality belongs to the object. On the other hand, it is distinct from the pain in that it is somehow concerned with the quality of the object, whereas the pain is not. If, then, there were only a single sensation-situation in which the 'sensation of pain' was the sensing and the sensation of hardness the sensum, we would have to admit that the sensum was distinct from the perceived quality, despite the fact that the relation between the sensum and the quality would then seem to present an insuperable problem. But I can see no sufficient reason for, but good reason against, thinking that is the case. For how could a sensation of pain possibly be a sensing or awareness of hardness? As we saw, the pain does not seem to involve any reference to hardness; indeed, we saw that I can feel the same pain without being aware of the hardness. Again, if it is either a sensum or a sensing, then, if our conclusion in regard to toothache is to be trusted, it is not a sensing but a sensum.

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[1] Cf. his article, "Prof. Alexander's Theory of Sense Perception", *Mind*, Oct., 1922:

"He [Alexander] asserts and I deny that sensa are identical with perceived features of physical existence" (386). "In knowing distinctions and relations between his own sensa, in knowing their changes and variations, he knows *corresponding* distinctions and relations in the domain of physical existence" (394).

We have not, then, discovered any reason for questioning our previous conclusion that the experience Reid describes involves two distinct sensations. One of these (the sensation of pain), if it involves a sensation-situation analysable into a sensing and à sensum at all, is a sensum, though not the kind of sensum with which we are at present concerned. The other (the sensation of hardness) is a function of sensing, the correlative sensum being in fact a quality of an object, though, if we merely sensed it, we would not know that it was. This is an instance of the kind of sensum whose status we are to discuss. So far we can only say that it is what we ordinarily call a quality of an object.

3. VISUAL SHAPE AND SIZE.

Prof. Stout maintains[1] that when I look at any object, say, a sheet of paper, I experience a sensum having a certain shape and size. The sensum, he contends, differs both from the corresponding character of the physical object, and also from the perceptual appearance of this, i.e., both from what the object is (or is normally believed to be),. and also from what "it seems to be to the percipient in the act of perceiving it".[2] This must be the case, for whereas the physical object never, and the perceptual appearance only sometimes, varies with variations in the retinal image, the sensum invariably varies in shape and size according as the shape and size of the retinal image varies.[3] Prof. Stout's contention, then, is that when I perceive such an object as a sheet of paper, I experience a sensum having a shape and size corresponding to the shape and size of my retinal image, but often differing from both the real and also the apparent shape and size of the object. But though the sensum varies in shape and size according as the retinal image does, Prof. Stout obviously does not identify the sensum with the retinal image. It is more akin to the perceptual appearance; indeed, it is so like this that "ordinarily we do not discriminate them".[1] In any case it is included in what we perceive. (This clearly is not true of the retinal image; it forms no part of what we perceive.) Nevertheless its *distinctive* characteristic is that it varies in shape and size concomitantly with the retinal image.

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[1] See "Prof. Alexander's Theory of Sense Perception", esp. p. 387; Manual, Bk. 3, Pt. 2, Ch. 1, Sect. 3; Mind and Matter, Bk. 4, Ch. 4.

[2] Manual, p. 409.

[3] "Prof. Alexander's Theory of Sense Perception":
P. 387: "It is I take it . . . . the sensa."
P. 396: "In approaching . . . . regular way."
Pp. 402-3: "When we move . . . . circular."
Mind and Matter, p. 261.

It should be noted that Prof. Stout does not himself mention the retinal image. His position can, however, be described more briefly by doing so.

What precisely, then, is the nature and status of *this* sensum? I say *this* sensum because it may be quite different in nature and status from the sensa we have so far discussed. It will avoid confusion in discussing this question if we add yet another distinction to those already made by Prof. Stout. I therefore propose to distinguish between:

1. The real object—what on the basis of all our experience we ordinarily believe the object to be;
2. The perceptual appearance—what the object seems to be to the percipient while actually perceiving it;
3. The sense-presentation—what we actually see while perceiving the object;
4. The sensum—something experienced which varies in shape and size as the retinal image varies in shape and size.

That the second can be distinguished from the first is obvious wherever perception involves, and is known to involve, an element of illusion. Thus we know that the moon is really a spherical body comparable in size with the earth. But to the percipient it seems to be a small cream-coloured disc or crescent. The distinction between the second and the third is apparent in the case of the perception of any solid object. Naturally we can actually *see* only the side or sides turned towards us; but we do not take the object to consist only of these sides; it seems to be solid and to have a back. The distinction is particularly obvious where, despite the fact that the sense-presentation is veridical, the perceptual appearance involves an element of illusion. For instance, what we perceive as a solid cube may really have only the three sides we see. The distinction is important because, if it were not made, we would be liable to confuse the sensum with the sense-presentation. If Prof. Stout had made this distinction, he would have used the term 'sense-presentation' where he now speaks of the perceptual appearance. For it is that aspect of the perceptual appearance which we actually see, namely, the sense-presentation, to which he is referring when he says that the perceptual appearance does not necessarily vary in shape and size according as the retinal image does. Consequently, when referring to Prof. Stout, I propose to make this substitution. In doing so, I cannot misrepresent Prof. Stout's position, unless he confuses the sensum with the sense-presentation. In that case the misrepresentation will consist in attempting to present a confused position clearly.

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[1] Manual, 409.

I now propose to bring forward evidence to show that the sensum not only can be distinguished, but in many cases must be radically different, from the sense-presentation. The sensum, I would ask you to remember, is the experienced something which (according to Prof. Stout) varies in shape and size in accordance with variations in the shape and size of the retinal image, and may be quite different in nature from the sensa we have so far discussed.

Hold a penny with its circular surface at right angles to your line of vision. You see it as a circle, i.e., its sense-presentation is circular, and it will produce a circular image on the retina (or at any rate I propose for convenience to assume that it does). Consequently its sensum will also be circular. Now gradually turn the penny about its horizontal axis, so that the top edge of the penny becomes more distant than the bottom edge. The retinal image, and therefore the sensum also, will immediately cease to be circular, and (neglecting the thickness of the penny) become instead a series of ellipses with progressively shorter minor axes. But what do we see? For myself, I am satisfied that I continue to see a circle, progressively sloping back more and more, until, as it gets almost edge-on, the penny seems to be rather blurred, so that I do not see it as of any definite shape. If I watch this operation with my eyes wide open, I never, so far as I can tell, see it as an ellipse until it reaches the blurred stage. At that stage I am inclined to think that I can, if I decide to do so, see it as an ellipse—indeed as an ellipse whose minor axis bears whatever proportion I like to its major. Prior to the blurred stage, the only way in which I can see it as an ellipse is by almost closing my eyes. And then I see it as an ellipse whose minor axis also is at right angles to my line of vision. That is to say, by almost closing my eyes, I see an ellipse of the same shape as the retinal image, and with its surface at right angles to my line of vision. But, if I keep them open, I continue to see a circle even when the retinal image has become an ellipse with a very short minor axis. It is clear, therefore, at least in my own case, that, so far as shape is concerned, the sensum is often radically different from the sense-presentation.

In the case of size the difference is even more radical. Though beyond a certain distance an object seems distinctly smaller than when we are close to it, at all distances up to a certain distance it seems the same size; whereas of course the size of the retinal image, and therefore of the sensum, will vary with every variation in the distance. Thus I find that a penny seems the same size, i.e., the sense-presentation is the same size, at eight feet as at one foot, despite the fact that the retinal image, and therefore the sensum, will be only one-sixty-fourth of the size. The question then arises: At what distance is the sensum the same size as the sense-presentation? Are we to say that they are the same size at one foot, while at eight feet the sense-presentation is sixty-four times as large as the sensum? Or should we say that they are the same size at eight feet, but that at one foot the sensum is sixty-four times as large as the sense-presentation? Obviously our experience of the two cases supplies no reason for saying the one rather than the other, or for maintaining that it is at either of these distances rather than at any other that the sensum is the same size as the sense-presentation. The fact is that, though we can give the ratio between its size at one distance and at any other, we can never determine what size the sensum is at any distance. We could do so only if we assumed that its size bears a specific ratio (e.g., 30:1) to that of the retinal image. Such an assumption, however, would be entirely gratuitous; there is no reason why we should prefer one ratio to any other. There may be one single distance at which the sensum is the same size as the sense-presentation. But we have no means of determining at what distance.

Our discussion has carried us further than we anticipated. Not only does the sensum usually differ radically from the sense-presentation, especially as regards size, but, further, we can never know what size it is. Now it is surely strange to speak of experiencing a sensum of a certain shape and size when we have not a ghost of a notion as to what size it is. We have not to look far, however, to find further paradoxes. For instance, though the sensa in question are visual ones, it is clear that we cannot see them. We can almost justify this statement even when we take account only of shape. It is clear that, no matter whether the shape of the sensum and the sense-presentation is the same or different, we have no ground for saying we always see both the sensum and the sense-presentation. We can never by introspection discover that we are seeing both. Indeed, it seems, if not nonsense, at any rate absurd, to say that we see both; for we would be maintaining that we see, either in the same place, or one in front of the other, two different objects, either of the same or of different shapes. Nor can we at will see either the sensum or the sense-presentation, in the same way that we can see an ambiguous diagram as either of two different objects. Where the sensum differs in shape from the sense-presentation, we can arrive at the shape of the sensum only by calculations from optical data and principles. Where the sensum is the same shape as the sense-presentation, *if* there were only shape to be taken into account, we might say that in seeing the sense-presentation we also see the sensum. But even then we could not see that it was the sensum, we could determine whether it was or not only by a similar process of calculation. Consequently, even if we could leave size out of account, it would be misleading to say that we see the sensum; we ought to say that we see the shape of the sense-presentation, and calculate that the sensum will be the same shape.[1] But of course we cannot leave size out of account; for the sensum is said to have a certain shape *and size.* Now we have seen that the sensum can be the same size as the sense-presentation at one distance only, and we cannot tell at what distance. Consequently it is certain that it can seldom be true that in seeing the sense-presentation we are also seeing the sensum, and we are never entitled to say that it is true in any particular case—unless we maintain that we see the sensum, if either it forms part of the sense-presentation or the sense-presentation forms part of it. But it is surely absurd to say that we see it, if the sense-presentation forms a part only of it, and we see only the sense-presentation. It is equally absurd to say that we see the sensum, if it forms part of the sense-presentation, for we cannot tell what part. It is like saying that we see a person because we see a crowd we know he is in. We must conclude, then, that we cannot see the sensum unless it is identical with the sense-presentation. These two obviously cannot be identical if they differ either in shape or size. Though we can determine the shape of the sensum by calculation, and know that it is sometimes (though we do not know when) the same shape as the sense-presentation, we are never, *in perceiving* the sense-presentation, aware of the shape of the sensum; and we can never even calculate the size of the sensum, though we can calculate that it must always differ from that of the sense-presentation, except at one specific but unknown distance. Hence the sensum can seldom be identical with the sense-presentation, and we are never entitled to say that it is. Consequently, even when it is, we cannot say that we see the sensum, unless we are prepared to say that we see a person whom we cannot pick out in a crowd. In short, though the sensum in question is a visual sensum, we do not and cannot see it. It cannot therefore be sensed. Such knowledge of it as we can attain is arrived at by calculation from optical data and principles.

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[1] It may be objected that I have forgotten my admission that I see the penny as an ellipse (i.e., as the same shape as the retinal image, and therefore as the sensum), if I almost close my eyes. In that case, you may say, I see the sensum. But surely I see the object as it appears under those conditions, i.e., see the sense-presentation. I know that the sensum is the same shape only as the result of calculation.

What is this sensum? The answer seems to be: Nothing! If it exists at all, it must be either part of the sense-presentation or else a separate object. It seems absurd to say that it is part of the sense-presentation. That would imply, for instance, that within the circular sense-presentation of the penny there is an elliptical sensum whose major axis is either equal to, or to any extent less than, the diameter of the sense-presentation. Besides, we are not entitled to say that the sensum is part of the sense-presentation, for, for all that we know, the sensum may be larger than the sense-presentation. It seems equally absurd to say that it is a separate object. For we have no ground for supposing it to exist, and can form no conception of what it is, or of where it is. We must surely conclude that the sensum, in the sense of the experienced something which varies in shape and size in accordance with variations in the shape and size of the retinal image, is a figment.

We may be told that the shape and size of the retinal image must make some difference to our perception, and so there must be something experienced which varies as the retinal image varies. Suppose there is. We can have no idea what it is. We are not entitled to assert that it is an object having shape and size, let alone to assign any particular shape and size to it. We are not conscious of experiencing it. We cannot even say that the experience of it is mental. All we are entitled to say is that the shape and size of the retinal image is one of the factors (or a manifestation of one of the factors) which go to determine the shape and size of the sense-presentation. If we are to speak of a sensum having shape and size, we must recognise that what we have so far in our discussion of visual shape and size called a sensum is a pure fiction, and identify the sensum with the sense-presentation or perceptual appearance. But then the sensum does not necessarily vary in shape and size in accordance with variations in the shape and size of the retinal image.

I have said that we must identify the sensum with the sense-presentation *or* perceptual appearance because it is no longer necessary to distinguish between these. The distinction was necessary only in order to guard against the confusion of the sensum, in the sense we have discarded, with the aspect of the perceptual appearance which we actually see. The sense-presentation is simply an abstraction from the perceptual appearance—that part of the perceptual appearance which is actually seen, as opposed to 'taken for granted'. The perceptual appearance is a sensum in the same sense in which colour and hardness are sensa, except that it is not, like these, an aspect only of what we perceive, but the complete perceived object. Shape and size are perceived as its qualities, just as colour is. Strictly, then, it is shape and size which are sensa in the same sense in which colour and hardness are.

(*To be concluded.*)

REVIEWS.

THE PHILOSOPHICAL BASES OF THEISM. By G. Dawes Hicks. George Allen and Unwin, Ltd., London; 1937. Pp. 272. Price 8s. 6d.

Professor Dawes Hicks has a twofold object in his "The Philosophical Bases of Theism", first to establish against the mystics the relevance of philosophy to religion, secondly to defend theism against its two principal enemies, which he identifies as naturalism and pantheism. Philosophy, he argues, is of service to religion because it is in a position to examine critically, and to reformulate, the inadequate symbols in terms of which religious truths tend to be expressed. "In interpreting religious experience", he says, "the notions which ordinary thought has at its disposal must, by the very necessity of the case, be meagre and thoroughly inadequate" (p. 48). More than this, however, philosophy must always be of vital interest to religion, because the philosopher by the very nature of the task he is set cannot remain indifferent to theological questions. Philosophy, according to Dawes Hicks, can demonstrate that there are religious truths, and this is something which no reliance upon inward feelings could of itself determine.

Here he finds himself bound to oppose the powerful anti-intellectualist movement which scorns philosophy and even knowledge of any form as being inadequate to deal with what is spiritual. As the outstanding representative of this tendency he selects Schleiermacher, and devotes considerable space to a criticism of the ambiguities and vague expressions which constitute the stock-in-trade of such writers. The major difficulty is clearly indicated, that confronted merely with a special sort of feeling, we are in no position to describe it as a feeling of "dependence upon God" unless we have the prior information that supernatural beings exist, so that Schleiermacher is compelled, whether he likes it not, to develop a *theory* of God's nature in order to exhibit the character of such a feeling.

Thus Dawes Hicks draws attention to the fundamental issue for religion, whether supernatural beings exist, and it is in his attempt to prove that they do that the philosopher will be particularly interested. It is on the basis of a thoroughgoing dualism that Dawes Hicks attempts to demonstrate his theism. This is inevitably so, because to be distinguishable from pantheism, theism has to maintain that there is an absolute gulf between God and "nature", and then, to connect God and man, has to insist on the freedom of man from natural law, thus opposing naturalism or materialism.

Such a dualism might superficially appear to follow from that realist epistemology which Dawes Hicks did a great deal to establish with its insistence that what knows must be different from what is known, but it would be a striking example of "vicious intellectualism" to distort this "different" into "different *in all respects*". A simple difference of function is then unwarrantably converted into a logical dichotomy. The same applies to that distinction between "internal" and "external" worlds which bulks so large in the dualism propounded by Dawes Hicks. It should be evident that although our mental processes are internal when their relation to our own body is being considered, they are external to one another and to other bodies of various kinds. To suppose that our own skins can sunder categories is the very height of egoism. Yet this is what Dawes Hicks maintains: "Not only are the relations between phases and modes of the mental life entirely different from the relations observed to hold between constituents of the material world, but no one of these phases can be described save by means of terms quite other than those we employ in treating of the parts of an object" (p. 67). The difficulty with such an unrepentant dualism, of course, is to connect in any way what has been so irrevocably sundered and to indicate how, in these terms, even the compresence of minds and other things is possible, how they can form part of the one discourse, as they must even to be distinguished. This is a problem which never seems to come home with its full force to Dawes Hicks, and yet it is one which must be met if dualism is to be presented at this stage in the history of philosophy.

He apparently considers, however, that the gulf between minds and other things is so great that whatever the difficulties which arise in giving a satisfactory account of it, this fundamental division simply has to be accepted as an irreducible datum by the philosopher, as one of the common-sense assumptions which may present problems but simply have to be accepted. In support of such an attitude, he quotes common verbal distinctions. "We speak", he says, "of influencing a mind by argument but not of propelling it by physical energy; we speak of a mind being attracted by persuasive reasoning but not of its being attracted to the earth by the force of gravity" (p. 67). Here, as in connection with many of the other distinctions which lend plausibility to dualism, the force of human interests, the way in which they lead us to emphasise particularly certain qualities of a thing, and to treat the relation a thing has to us as constituting the thing, finds no recognition. Thus in connection with "internality", the distinction between what is inside and what is outside the confines of our bodies is one of special importance to us, so that when we talk of "the external world", we know perfectly well what we are talking about, viz., external *to us,* just as when we call a person an alien, this means of a race other than our race, but, of course, we are the alien from his point of view. To regard ourselves as a foreigner, or as part of other people's environment is difficult just on account of our strongly egocentric tendencies, but it is one of the main tasks of philosophy to correct this insularity by its insistence on the common features of things.

In the particular case of the motion of mind the same type of consideration applies. The point can be illustrated when it is remembered that the verbal distinctions on which Dawes Hicks relies apply equally to a great many things which he regards as being part of "the material world". Thus we do not speak of propelling blue by physical energy or of blue being attracted to the earth by the force of gravity; and yet Dawes Hicks correctly regards colours as being just as real as anything else. We have no hesitation, however, in speaking of a blue book being propelled across the room or falling to the floor, and would be forced to admit that the blue moved along with the rest of the book. Similarly, we talk of a human being as subject to the law of gravity; and when, for example, Jack and Jill fell down the hill, we assume that their minds were not left floating serenely on the hilltop. Both colours and minds always move as part of a bodily mass and at the same rate as the rest of that bodily mass. Since in considering questions of propulsion and gravitation, it is with the movements of such masses that we are concerned, we naturally refer to them by means of such characters as will most accurately determine the configuration of the bodily mass in question. Thus we speak of throwing a cricket ball into the air rather than of throwing a red into the air, because the reference to a bricket ball informs us of the approximate size and mass of the object in question, where the reference to redness would not. Similarly, there would be no objection to our referring to the initial velocity and the rate of acceleration of a mind, but since minds always fall along with, as a character of a certain part of, human bodies and share in the velocity and acceleration of that body, it is usually to the rate of motion of that body that we refer, just as we say the earth moves around the sun rather than that Sydney University moves around the sun, although both statements are true. It is all the while a matter of our interests; so that the fact that we do not usually refer very much to the physical relations of minds, and more especially to their movements through space, does not demonstrate that they do not have these relations.

Dawes Hicks' further argument is equally unsatisfactory. He endeavours to prove the "quite unique unity" of the mind, on the ground that in knowing a series of events, "the members of the series must be grasped not merely one by one as they come and go, but, as a whole, in their totality". This, he argues, makes it impossible for the subject to be itself a series of events. "We may speak of the cognitive act in question as being a mental event, but by the very qualification 'mental' we imply that it is an event in the life of a mind possessing a genuine unity. Otherwise it would, at the most, be a series of awarenesses . . . . but certainly not the awareness of a series" (p. 68).

What may be doubted, however, is whether minds are at all peculiar in this regard. Dawes Hicks seems to think that there is some opposition between being an event and being continuous; but again the question of varying emphasis arises. In talking of a thing as an event, we certainly suggest that it occurs at a certain time rather than during a certain time; but for it to occur at all it must persist during a certain time, and we distinguish earlier and later phases in events, i.e., we recognise that an event can persist while other things "come and go". On the other hand, any continuity is a particular continuity, i.e., it has a place in a time-series as an event in wider continuities. By the use of the phrase "a genuine unity", Dawes Hicks suggests that there is something special about the continuity of the mind, and that other unities are by comparison spurious, but the nature of this "genuineness" is nowhere indicated, and he is in fact forced to admit that the unity involved is of a quite ordinary kind, of that kind in which events (each itself a "genuine unity") can be separately discriminated. By calling them "mental" we no more insinuate that they have a special relation to a unity than in calling them "red" we make the same suggestion. The position is rather confused, because mind is so often merely regarded as that in which "mental states" occur; so that in calling an event "mental" we may appear to be saying where it is rather than what it is and hence to be merely placing it in a series and not qualifying it; whereas if we call an event "red" then although we are automatically (by the use of the copula) placing it in a series, pointing to its persistence by the indication of the fact that it has characters, the emphasis is upon the particular characters involved and not the particular locality of the event described as "red". If, however, the word "mental" has a meaning, i.e., if it refers to definite characteristics, then to discover that an event is mental is just to discover that it has these characters; and if mental events have anything especially systematic about them, this fact would have to be conveyed in a further proposition, e.g., that "all mental events are part of a genuine unity", and certainly is not implied merely in calling them mental.

The arguments presented by Dawes Hicks to show that "the subject" is categorically other than "the object" cannot be accounted satisfactory and, indeed, the nature of the categories is such that any such arguments are bound to be unavailing. In this particular case, there is the special point that the distinction between subject and object is not a distinction in character, although the special fact that not everything which is known can know may make it appear that it is. The matter can be illuminated by a comparison with the relation between artist and model. When we call a man a model we think of him as being painted, as when we call a thing an object we think of it as being known, but many models are themselves painters, so that there are not mutually exclusive classes of painters and models. The differentiation of artist and model is possible because the two exercise a different function in a particular situation, and we can in the same way distinguish subject and object under a particular set of circumstances, but it is nothing short of astounding that Dawes Hicks can argue that "whenever we are tempted to treat the knowledge of an object as though it, too, were an object to be known we are violating in an unmistakable manner one of the clearest deliverances of experience" (p. 66). Any theory which involves the distinction between living through and knowing has first of all to demonstrate its necessity, the onus of proof is clearly on its side because it is by no means a clear deliverance that there are two sorts of knowledge involved, and has to account for the fact that we are sometimes quite convinced that something which is actually one of our own mental processes exists outside our own bodies, the phenomena of paranoia merely exaggerating what is at any time a common enough tendency.

Dawes Hicks has at least the merit of consistency, and his realisation of the implications of his dualism leads him to criticise the traditional cosmological argument. He sees that it is impossible for a thorough-going dualist to speak of the mental as being caused by or as causing what is not mental. "Whenever the entities under consideration cannot be regarded as objects", he says, "but can be construed or interpreted only in a way other than that which is appropriate in the case of objects, there the application of the thought of causal nexus is illegitimate" (p. 168). In particular, it is grossly inappropriate to conceive God as a cause because "this would imply either a change in something already existing other than God, or else a change in God Himself, whereby from a condition of non-creativeness God passed into one of creativeness. And each of these alternatives is clearly contradictory" (p. 167). The difficulty, however, admitting that it is impossible to regard minds in general and God in particular as a cause on a strictly dualist logic, is to express the matter in any terms whatever which shall not be subject to the same type of objection. Even to say that both minds and other things are dependent upon God infringes against the principle that minds cannot be conceived in the same terms as whatever is non-mental. This particular problem finds expression in Dawes Hicks' dual use of the word "nature", now as "the sum of interrelated objects with which natural science is concerned" (p. 180), now as something in which minds can be said to have "a place". This is no accidental ambiguity, because if minds are to be regarded as part of a system dependent upon God, they must have something in common with the rest of that system, they must be natural when contrasted with the supernatural, while, on the contrary, to demonstrate that minds are not subject to natural laws, it is necessary to regard every mind as being in some sense supernatural. It is only by such shifts of meaning that Dawes

Hicks can avoid pantheism, to which he can rightly object that it does not account for evil, but which is logically superior in its insistence that there can be only one sort of reality.

Notwithstanding his criticism of the traditional cosmological argument, Dawes Hicks considers that some sort of cosmological argument is the only possible speculative proof of God's existence, although, given this, the details can be filled in and the spirituality of such a being can be demonstrated by the argument from design. The speculative argument he expresses in terms of intelligibility, and it is, in his eyes, the spectacle of a world which cries out for explanation, which is contingent in terms of "rationality" and not of causality, which demonstrates the existence of God. "Acknowledging, as we cannot help doing", he says, "the existence of the world of nature, we are logically driven to acknowledge that there is real existence beyond nature unless, indeed, we are prepared to rest in an ultimate inexplicability, and to relinquish the attempt to frame any intelligible conception of nature at all" (p. 180). Again, there is ambiguity in the phrase "the world of nature". We may be all prepared to admit that there are natural things, or more simply, that there are *things,* but this is a very different matter from admitting that there is something describable as "the world of nature", constituting a totality and permitting comparison with other worlds. Any naturalist who admits this is quite unnecessarily running his head into a noose, because to affirm such a totality is at once to admit that some things are not "natural". The naturalist is logically bound to take his stand against the whole conception of a totality, but this is a possibility which does not seem to occur to Dawes Hicks. Such a naturalist, I suppose, would still be subject to the criticism that he "is virtually implying that there is just no reason why the constituents of nature are as they are, why there are so many different kinds of them, or why they have the specific characteristics they are found to have, except, indeed, the 'reason' which is in truth no reason, that such 'happens to be the case'. He is implying, in other words, that there might just as well have been no world at all, or a world totally other than the actual one. And this means . . . . that we are simply acquiescing in 'brute fact' not because in the present state of our knowledge we can do no otherwise, but on the ground that there is not and cannot be any explanation—on the ground, that is to say, that unintelligible mystery subsists at the very heart of things" (pp. 180-1). Ambiguity again! If the naturalist by 'what happens to be the case' meant to affirm that things have no causes, but happen at random, Dawes Hicks might have some reason for his strictures, but we may very strongly doubt whether this position could be described as naturalism. If, on the contrary, the naturalist is only drawing attention to the fact that any cause or explanation is itself a 'brute fact', i.e., something which actually exists and about which further questions could be asked, what is unintelligible and mysterious about this? If all things are dependent upon God, then this is a brute fact like any other; this 'happens to be the case' in the sense that this *is* the case. The big difference between the naturalist and the theist is that the naturalist maintains that any explanation can itself be the subject for inquiry, whereas the theist is compelled to assert brute facts of which he can give no further account. The claim that to postulate God as a cause is to dispel all mystery is reminiscent of Shadworth Hodgson's recipe for philosophy: "Whatever you are *totally* ignorant of, assert to be the explanation of everything else."

In support of his distinction between rational explanatory principles and irrational brute facts, Dawes Hicks seeks support in mathematics, but expresses himself in a particularly unfortunate way. "By mathematics", he says, "we can never prove that anything exists, nor can any character which we are led to assign to existent realities ever influence, or in any way affect, the truths of mathematics. The two realms are, of course, related (if they were not the scientist would find mathematics of no use to him), but the relation is of such a kind as to render the possibility of conflict out of the question" (p. 192). Quite so—the *truths* of mathematics remain, whatever happens, but so do any other *truths*; nothing which ever happens can affect the truth that it was cloudy last night. The real question is whether mathematical *falsities* can clash with the facts; and they can. We can disprove a false trignometrical formula by measuring a hill; such a formula asserts that certain brute facts are true, and if only one of them is false, the formula is immediately proved false. No more in mathematics than anywhere else is there any opposition between principle and brute fact, but all we have in either case is a general mode of operation.

It would appear, then, that Dawes Hicks is unlikely to convert many naturalists, and that he absurdly over-simplifies the naturalist position. While this is so, his arguments will very often assist the naturalist, as well as the theologian, to clarify his position. He argues quite acutely, for example, against the confusions attendant upon atomism, his rejection of the view that all change is quantitative being forcefully expressed and cogently argued. Again, however, he apparently believes that the defeat of atomism carries with it the downfall of determinism, whereas determinism can quite consistently recognise qualitative change. In spite of his rejection of some of the cruder errors of professional scientists, there is altogether too much uncritical acceptance, on mere authority, of whatever in "science" seems to suit his immediate purposes —vitalism, the great mathematician, etc. One can wish that philosophers would lose some of that reverential awe which overcomes them at the sight of "practical scientists". Philosophy fast loses its historic position as the science of sciences and becomes the servile flatterer of any scientist who deigns to pay it the least attention.

J. A. PASSMORE.

HUMAN AFFAIRS. Planned and edited by R. B. Cattell, J. Cohen and R. M. W. Travers. Macmillan and Co., London. 360 pp. 10s. 6d. net.

This book which consists of sixteen essays contributed by prominent social scientists is, the editors announce, a first attempt to produce cooperatively a "philosophy of human science" and to show how the various "human sciences" bear upon the solution of social problems. If the reception given to it is encouraging, it is to be followed by a periodical of the same title devoted to a further consideration of the social bearing of science. The editors, however, make claims which some at least of the contributors do not attempt to fulfil. Much of the material presented is not of any special relevance to the philosophy of science; it is not clear, for example, how Katz's discussion of the food-habits of various peoples, or Lord Raglan's discussion of race, can be thought to bear upon philosophy. But, apart from this, one of the most striking defects of the book is the absence in almost all the papers of any trace of philosophic habit. A philosopher would consider it essential in approaching such a subject, to scrutinise the terms, assumptions or categories prevalent in popular thinking about society and the situation of science therein; what prevails throughout these essays is a most uncritical use of the conceptions current in commonplace social theorising. For instance, the blessed word of the book is "adjustment"; we are told repeatedly that what is wrong with contemporary affairs is that they are "maladjusted" and that "adjustment" is what, guided by science, we are to aim at. Not one writer tells us explicitly what either of these terms is supposed to mean; how we are to recognise a "maladjustment" when we see one, or why a situation of "adjustment" is to be preferred to its opposite. There are a number of other terms of the same sort, like "application" (of scientific knowledge), "happiness", "good citizenship" which philosophers have long since learned to treat with the greatest caution and distrust, but which many of these writers seem to regard as transparent and as forming a certain and safe basis for theory.

It would not be quite fair to say that the claims of philosophy are wholly ignored; Cattell even regards it as a merit of a vocational education that it fosters philosophy: "Anyone who has heard farmers in a country tavern discussing a common sheep will realise what a wealth of science, literature and philosophy can be conveyed in a vocational education" (p. 152); a remark which indicates what conception of philosophy can in these days be held by a rising young educationist. The official exposition of the relation of philosophy to the social sciences is undertaken by McDougall, but he also begs the questions which philosophers would like to see discussed and which the other papers constantly bring to mind. Broadly, his position is the old one that it is the busisness of the philosopher to provide "ideals", to announce what ought to be done while the scientist shows us how to do it. What we should expect, however, if philosophy is a criticism of the sciences, is an account of the way in which societies do change with a consideration in particular of the extent to which "ideals" and moral teaching and scientific instruction can affect the course of social development. There seems to be little point in prescribing an historical "function" for the philosopher or scientist until it has been discovered just in what way science does interact with other historic forces. The philosophy which devotes itself to scolding and exhorting (in McDougall's own manner) may find that it is historically negligible.

In holding that it is the function of the sciences to guide and to exhort, McDougall is merely making explicit the popular philosophy of the other contributors. The difficulties of the position are not carefully enough considered. The editors do admit that "many a scientist spends his days solving some complex problem of life only to find when he does so that he cannot make people heed the usefulness of his discovery" (p. 6). This, of course, is only to say that there is a variety of interests and that we are not all satisfied with the "solution" to our problems that may commend itself to some scientist. But the main point that scientists, eager to help, have to face is that it is not from a consideration of what is "good for him" or what is "good for man" that a man commonly acts; a calculation of our "aims" or "needs" is not the motive power of history. It is this point that Malinowski runs against when he says (p. 226) that "the overdevelopment of mechanical science and its applications have comparatively outgrown the progress of our knowledge of how to adjust our efficiency to really human aims and needs". But, we should submit, this account of historical progress as a matter of adjusting efficiency to aims, "really human" or otherwise, has no verisimilitude at all. It would seem rather to be the case that men are generally unaware of what their aims are, or rather, that the aims that they profess are an after-thought which conceals the real tendencies of historical development. It should surely now be an historical commonplace that the actual direction of history is in ludicrous contrast to the "aims" and other pseudo-explanations of their activities which human beings fabricate. Or if that is not to be accepted as a commonplace, there is, at any rate, nothing in the book to show that social development would in any way be affected by any attempt to inject new "ideals" into the social ferment, the more especially as the "ideals" in question are already circulating with sufficient freedom.

We should maintain, in effect, that it is not that our means are not adjusted to our "real" ends, but that the "ends" we profess are of small historical account and that what, in a grandiose and self-laudatory way, we are fond of calling our means *are* our "ends"—are, that is, what we are really interested in. Malinowski follows up his first point by another, that "modern civilisation is a gigantic hypertrophy of material objects and contemporary man will still have to fight his battle in order to reassert his dominance over the thing" (p. 227). But he again brings no evidence to show that man ever had "dominance over the thing"; in fact, it was precisely his reflexion a little before that, in Polynesian society, "the house dominates [the] whole mode of life" that led to a consideration of the state of modern society. The fact seems to be, in contrast with Malinowski's conception of "real human aims" that we are interested in sorts of activities for their own sakes; we are interested, for example, in industrial production and in expanding it, in using and improving tools and machines, in travelling faster, in operating with and upon the various things that we find at hand. No doubt, it gives us immense satisfaction to *say* that the profit-producing machines we are operating are our means of promoting light and culture, and all sorts of elevated "ends", but actually we are interested in the things themselves. All that the phrase "dominance by the thing" seems to mean is that we live in certain ways, within certain accepted institutions, that we maintain certain modes of production, forms of religion and of art and so on because those things themselves interest us and not (whatever we might say) because we are considering ultimate social effects or some remote condition of social "adjustment". This would account also for the social "irresponsibility" of physical scientists (upon which the editors comment); it is only what is characteristic of any body of men working within any organisation—they carry on, for its own intrinsic interest, a certain regular type of activity and it is not their business, nor is it possible for them, to take a total social view or to aim at a final equilibrium. As a recent writer puts it, "the ordinary man has habits but not ideals"; at any rate, it is habit which is of importance for the social theorist. For it is the existence of forms of organisation or institutions which draw in and utilise human material which makes prediction in the field of society possible, which enables us to deal with questions of social development apart from individual human decisions. Malinowski recognises that his defence of determinism in social science can be carried on only in terms of institutions, but he does not admit the further point that his emphasis of "ideals" and "aims" would make it impossible to have any social science independent of psychology.

To this line of argument the reply would presumably be made that it is the social scientist who now proposes to take the total view and consider the final equilibrium. Here again, we may retort, what the social scientist thinks he is doing need be in no correspondence with what he actually does. But it seems that scientists, in their social thinking, will never learn to avoid the "experimentalist's fallacy", the habit of thinking of themselves as above the social scene with the rest of society as material to which their knowledge is to be applied. The way in which the editors contrast the ignorant but powerful politician with the learned but powerless scientist is one illustration amongst a number of the view these writers take of their relation to society—the selection of the politician as the object of attack showing that they regard "power" as a magic wand which enables its fortunate possessor to manipulate society as he likes. It is also frequently suggested, as we should expect, that the neutrality of the scientist should raise him above the vulgar, political brawl, but it should be obvious that if there are any social institutions with an historical tendency opposed to that of scientific study, there must inevitably be struggle between them. The editors' conclusion on this matter deserves quotation: "The social sciences other than economics are, however, apparently at any rate politically neutral, and it is hoped that a great deal can be done by stressing the possibilities in these spheres. A coal owner would not object to a scientific study of delinquency, whilst even an armament manufacturer might, in a momentary weakness, endow a medical research institute" (p. 17). From this it would seem to follow that science will have its greatest social triumphs when it is universally inoffensive. But the fundamental point is that no matter how much scientific knowledge is acquired and distributed, that in itself will do nothing to remove the conflict of interests and historical tendencies from which social "maladjustments" arise.

These considerations, though discouraging to the writers of this book, are consoling to those of us who are not convinced of the moral and political acumen of most social scientists, and do not regret, therefore, that their influence is slight. The reference to coal owners and armament manufacturers is instructive: what little tinkering the scientist is able to effect will be, as the editors admit, within the limits permitted by the dominant social forces and thus we might expect that any equilibrium that social scientists can bring about will be by "adjusting" the weaker interests to the demands of the stronger. But whatever may be the reactions of the coal owner to the statement that "each and every problem of social life will yield to intelligent and patient investigation by the psychologist, the economist or the biologist" (p. 6), the more important fact is that no one with any intelligence, pride and independence will want solutions provided for him by the scientist or by anyone else. Nor will he easily be persuaded to give up his own demands by the scientist's claim to an insight into "really human aims and needs"; this may appear to him as a ruse of political seduction. This does not mean, of course, that the information provided by the scientist is of no relevance to our social activities; each of us will absorb what information we can and will, no doubt, make whatever use of it our interests and habits allow. But this is the same as to say that we shall find what solutions we can by thinking, by our own initiative and critical activity. This, it is at least conceivable, is culturally preferable to having scientific knowledge applied *to* us, as is the case when we consent to accept some solution handed down to us in the name of "really human needs" or some general social "good".

It would appear from these pages, however, that scientists do not, on the whole, consider it their social task to promote widespread thinking; they regard science in its relation to society rather as an art of healing. We learn, with some consternation, that even our very feeding is a "problem" for the unfortunate social scientist who seems, with complete lack of humour, to have taken upon himself all the sorrows and burdens of mankind. It is not surprising that the scientist should erect all our lightest doings into "problems" for himself, since widespread thinking is not conducive to the stability, "adjustment", "comfort" which seem to be especially desired, and, again, if the scientist insists that our problems are *his* problems, it is because he thinks us to be incapable of dealing with them ourselves. To this latter point we must return. But quite the most offensive thing in the book is

McDougall's ill-tempered attack upon philosophers for their hesitation in accepting the hasty and illiberal policies some social theorists propose. "The men of science who have learnt how greatly to improve many races of plants and animals . . . plaintively allege that they do not know what kinds of change of human species would be improvements. And the philosophers, repudiating the example of Plato, excuse themselves under the plea of ignorance, or find some moral alibi in ancient catchwords, such as that all men are created equal and alike, or that unrestricted reproduction is a fundamental right of every man, of every moron and of every criminal, a right which must not be infringed even though we all be dragged down together to the dust with the ruin of every ideal and every prospect of mankind" (p. 345). We ask (but are not told) how McDougall would defend the logic and the psychology upon which the politics of the *Republic* is based. But this is the very negation of philosophy; however vigorously a philosopher may argue, he neither expects nor desires that what he says should finish discussion; certainly he would consider it no part of the philosophical temper to desire to force his conclusions or "ideals" upon someone else. So, as against McDougall's conception of the philosopher-policeman, we may prefer to emphasise a somewhat different Platonic view —that of philosophy as the practice and promotion of the "life of criticism". The man who lives up to someone else's ideals is not responsible, is not even mature. There may be arguments to support McDougall's politics, but it is certain that philosophy can consistently supply none of them.

Not many of the papers, in fact, lend any support to the editorial claim that science can emancipate itself from political issues; it is rather made clear that in politics the editors have mistaken ignorance for innocence. It is absurd on the face of it to say that there could be any sensible discussion of social structure and development which ignored politics, and the political limitations of the writers betray themselves at almost every turn. Thus, while it is not unexpected that Cattell should press the claims of vocational education, the grounds he gives for it are alarming. He speaks of "the democratic philosophy which sees cultural value in other than literary or 'academic' subjects, and maintains that all forms of learning should have equal status". The reply to this, of course, is that not all forms of learning have equal social status or significance; there are certain subjects which a person must have studied if he is to be capable of thinking about, and exercising initiative in, public and political affairs. An education is democratic in so far as it makes available to all the sort of study or thinking that is essential to the grasp of, and participation in, fundamental social movements. And, we should claim, vocational education is undemocratic since it provides for a great number a sort of training that makes them incapable of social spontaneity and initiative. It has the final effect of forcing upon these people someone else's "solution" of their social problems.

If Chambers, who gives an account of the promise and achievements of industrial psychology, is correct when he says that "whatever [a boy's] nature, he usually needs help to find the first rung of the ladder which shall lead him either to the status of a good citizen or to that of an economic misfit" (p. 73), that is an unfortunate situation, demoralising alike to the scientist and to the "subject". Without agitating here the question of natural vocation, what was said in general about scientific "service" may be repeated as regards vocational guidance. It is difficult to imagine a high-spirited boy with active interests and initiative submitting to be led to the "first rung". Anyone whose education has awakened a multiplicity of interests, whose training has been sufficiently broad and critical to enable him to see where his interests can find an outlet, should gravitate naturally to the sort of work which attracts him most. And even if it is the case, as, no doubt, it is, that we are frequently mistaken in our estimation of what our real interests and capacities are, it has not been shown that it is not a good thing that we should *discovcr ourselves* by actual experiment and experience with different sorts of activities. Such unhappy experiment will often develop a depth of character preferable to the juvenile "happiness" and smugness that so often distinguish the "well-adapted" man, and will, certainly, promote a wider social experience and capacity for characterising the various kinds of social life. Much is said in this book about the "sickness" of society, and though we may, in general, deprecate this line of approach on the part of scientists, it does seem to be an indication of sickness, of some educational and social debility, that the services of vocational science should be so much in demand. We should expect that it is mainly amongst those who have been over-strained and stupified by their education, or are helpless, for some other reason, in the face of their environment, or have been hypnotised by the myth of "science", that the enterprises of the vocational psychologist will greatly flourish. But any "solution" that the psychologist can produce will be at the expense of originality and free, experimental living, and will conceal that absence of social and intellectual energy out of which these "social sciences" arise.

It is here, of course, more than anywhere else that the mysterious conception of "maladjustment" most confidently appears. Despite the obscurity in which the whole matter remains, it is clear at least that "adjustment" is taken to exclude discontent and waste and to promote comfort and efficiency. Now it is easy enough to describe a sense of "maladjustment" in which it may be said that it is the condition of any cultural progress; if, as Dewey has always insisted, all thinking begins with a problem, with a frustration of desire, it would seem to follow that "maladjustment" will be the source of any thinking. Chambers' account of the measures taken to produce "efficiency" in different occupations makes it plain, however, that thinking is not desired, since we do those things we have to think about less efficiently than purely "mechanical" operations. It has already been hinted that the industrial psychologist is little interested in self-discovery and in self-development (except, possibly, in the sense of "climbing the ladder"), and this is only to be expected from one whose thought is so much in terms of innate capacities and pre-determination. And the motive which dominates the thought of all these scientists is of special importance here: being concerned so much with *leading* society and preserving it from instability, they wish to eliminate any factor that may be incalculable, to exclude "fecklessness" (as one contributor describes it) and to insist upon what is safe and prudential. No doubt, the elimination of fecklessness is essential if the peace of mind of scientists and other social saviours is to be preserved; it is, however, another question whether from the consideration of individual comfort any cultural or social advance could come.

When the scientist argues that large numbers, from ignorance and other causes, are helpless, this is largely true, but it is a condition that cannot be met by guidance, and especially by individual guidance. The "maladjustments" that arise from the pressure of broad social forces upon large masses are by no means negligible, and there is no reason why they should be deplored, provided those masses have the interest and the knowledge to act upon their environment. Especially by the psychologists are these larger social issues ignored, and there is no reason for believing that individual guidance will dissipate the "murky yet tolerable fancies" of which Chambers speaks in connection with "maladjustment". The divorce from large social movements which, we suggest, scientific planning and guidance tend to accentuate, can produce fantasies of its own; and, indeed, no fantasy of the discontented office-boy is half so murky as the fantastic utopianism and voluntarism these scientists proclaim. When, for instance, Miller (writing on *Neurosis and Civilisation*) looks forward to the change from "human societies instinct-ridden" to "human society in which reason will at long last have achieved autonomy" (p. 277), we are seeing the result of the scientist's snobbish, "rational" refusal to accept and embrace the actual impulses, interests, activities that constitute any society and to identify himself as a participant with some one movement in the social ferment.

Perhaps it is not to be assumed that the complacency that characterises these writers is a feature of all scientific thought about society. Since it is the object of the book to interest the public in the social possibilities of science, it was natural to attempt to present as confident a front as possible. But in its emphasis on safety and consolidation, the book merely re-echoes what has undoubtedly become the keynote of contemporary political speculation. It is doubtful, however, whether there is any promise in this attitude: rather it seems to be a deterioration, to involve a lowering of intellectual and moral standards, a lessening of political energy and spontaneity in the interests of some supposed social "welfare". The authors are concerned, as is natural, with the menace of fascism, and do not hesitate to condemn it; they do not see, however, that in their concern with "adjustments", with guidance, with "real human needs", they are infected with the sort of totalitarianism they are professing to oppose.

P. H. PARTRIDGE.

PLATO'S CONCEPTION OF PHILOSOPHY. By H. Gauss, Ph.D., Lecturer in Philosophy at the University of Basel. Macmillan. 6s. net.

Dr. Gauss is a Swiss philosopher who has been touched by the glamour of the Anglo-Platonic religious tradition, and, in token of his debt to Dean Inge, Professor Clement Webb, and Professor A. E. Taylor, has written this essay (most efficiently) in their native language. He is in full revolt against the German conception of Plato as a professorial rationalist with a complete metaphysical system, and sees him rather as a seeker, in love with wisdom, and under a vow to seek no other, but understanding that complete knowledge is not vouchsafed to humanity, and convinced that such knowledge as is attainable is a mere "by-product" of a dedicated way of life. It is with *conceptions* of philosophy that he is primarily concerned: and his publishers do him an ill turn when they announce on the cover "a reasoned account of Plato's teaching". He never attempts anything of the kind: he is concerned with the spirit rather than with the content of Plato's inquiry. The interest of the work is, indeed, not only historical; it extends to the examination of a philosopher's credentials; but it is none the less considerable, for every philosopher finds it helpful to turn aside now and again from his philosophising, as Plato did in some of his magnificent digressions, and to ask in good earnest what he is trying to do.

Having examined, with some penetration, alternative conceptions of philosophy—the naturalist, the ethico-pragmatic, the scholastic, and the critical—and having shown how Plato does justice to the fruitful elements of all without surrendering to any, Dr. Gauss returns to Plato's own conception, and from his fresh angle of approach he brings out excellently points which are indeed familiar to students of Plato, but have rarely received such a significant emphasis: that philosophy is a passionate quest for what we do not know, that the quest is never complete, that such knowledge as we have is sandwiched between the dark mystery of nature and the bright mystery of the transcendent, that it grows dialectically through the contact of different sorts of minds, that there is only a "more" and a "less" of it, never an utter lack or complete fulfilment, that the philosophic attitude is communicated by example rather than by teaching, and finally that through the convergence of reality and value in the Form of the Good the philosopher's quest expands till it becomes the religious quest of the whole man, and the progressive mastery of dialectic is illumined by the insight of wisdom.

One result of this approach is that an unaccustomed emphasis is thrown on the sceptical element in Plato's philosophy. It has never before been so clear how the Academy became in Hellenistic times the home of scepticism: indeed, one of Dr. Gauss's most eloquent passages (pp. 182 ff) reads as if Carneades had returned to life as a communicant of the Church of England. It is not so much that he deliberately slurs the rationalistic factors: as he says, "philosophy is a lifelong devotion to the Idea of Truth". It is rather that the selection of material demanded by the theme excludes an exposition of Plato's more formal and positive teaching. Perhaps the most seriously misleading feature of the essay is that in stressing the unknowable transcendence of the real it tends to slur the distinction between the universal and the particular, and suggests that knowledge which is not ultimate is not certain either. This is certainly to misstate Plato's real position, which is that apprehension of universal relations differs *toto caelo* from apprehension of particular relations in point of certainty, and that such doubt as remains affects not those truths themselves, but their place in a metaphysical order dominated by the Good. He admits the scepticism of the twilight, and the scepticism of the sun-gazer: but there is the intermediate world "on which truth and being are shining", and here there is room for sure and certain knowledge.

However, Dr. Gauss is earnest, fresh, and original: his treatment often throws a new light on familiar passages: and his enthusiasm for Plato and for philosophy is such as to put fresh heart into all who share them.

A. BOYCE GIBSON.

PSYCHOLOGY AND HEALTH. By H. Banister, M.Sc., Ph.D. Cambridge University Press, 1935. Pp. 245. Price 7s. 6d.

In the Preface to this work Banister states that "the skilled physician must be a psychologist with a knowledge of the workings of that most intricate of things, the human mind" (p. vii). Such knowledge is needed, he maintains, for the treatment of purely organic diseases as well as for mental disorders. "It is probably no exaggeration to say that there is no purely physical illness; that every physical ailment is complicated more or less by mental factors" (p. 96). Considering this and the examples given of erroneous diagnosis by doctors ignorant of mental disorders, we can agree with Banister that the physician "should at least know how to avoid treating his patients wrongly and should be able to advise them where they can go for assistance" (pp. 7, 8). But Banister goes further than this, and, following T. A. Ross ("The Common Neuroses"), considers that the physician is the person best fitted to treat the majority of psychoneuroses. A case could perhaps be made out for such a claim. The special opportunities which a doctor has, the respect in which he is held, and the medical knowledge which should safeguard him from mistaking an organic for a mental disorder and *vice versa,* might be held to fit him for the work of psycho-therapy. Banister, however, makes no such attempt to justify his statement. His preference of the practising physician to the specialist, except in the more complicated cases, is connected with his general cautious attitude, shown in his statement (p. 213): "The golden rule in psycho-therapy is to go no deeper in analysis than is necessary", and this, of course, is connected with a particular view of the origin of and treatment required for mental disorders in general. He is especially concerned to find an alternative to the Freudian theory of the causes of hysteria, etc., though at the same time anxious to admit its "truth as a general principle", even though it is not the "whole truth". Such a half-and-half attitude to theories is not compatible with argument, and no definite conclusion can be said to emerge from the book as a whole, which indeed attempts to deal with too many aspects of the subject for its size. Thus there are chapters dealing with infantile sexuality, sexuality in general, the problem child, mental defect, all the mental disorders—dissociation, conversion hysteria, etc., the theories of Janet, Freud, Jung and Adler, hypnosis and suggestion, also general chapters on the invalid, and troubles of the healthy, and an outline of the nature of treatment.

Banister explains the causes and the treatment of mental disorders in terms of his central notion of Sentiment formation. He presents this as an alternative to the Freudian notion of Repression, which, he holds, the Freudians apply to too many mental processes. It is certainly a valid criticism of Freud's theory of the mind that it does not sufficiently recognise complexity of motives, that it explains as activities of repressed forces many that are due to independent forces, operating quite freely. But Banister extends this criticism in such a way as to obscure the most important findings of Freud in the field of psycho-pathology. He admits the existence of Repression and speaks of the "complexes" as "repressed sentiments", but his regular explanation of the mental disorders neglects repression and is in terms of his theory of sentiments. Thus it is said that the patient has not properly integrated his sentiments, his central sentiment is an unworthy one, or was formed about some object which has perished, so that another sentiment must now be brought to the fore. But Banister nowhere establishes the point that is implied here, that a struggle between sentiments fought out on the plane of "consciousness", and not involving repression, can lead to the symptoms and troubles of hysteria, etc.

A theory of Sentiments is not incompatible with repression, as McDougall has shown, but quite apart from its value for psycho-therapy, Banister's general theory of the sentiments is faulty, involving an atomic theory of the mind. He presents the pre-Freudian "idea of integration, the idea of the inevitability of the combination in life of the more elementary to form the more complex. In particular, in the development of mental life, we have this integration manifested in the building up of sentiments round objects out of the more elementary instinctive tendencies or emotions. The emotions which are aroused by, or are associated with, the objects are probably the links in the integration. I suggest that there is innate in every animal such a tendency. It seems to me so important that I would have it given a specific name, viz., sentiment-formation tendency" (p. 232). Such postulation of special principles can only keep theory back by presenting as a solution what is a re-statement of the fact to be explained, namely, that these primary tendencies do interact in certain ways, so as to form sentiments. No special relating-principle is required to explain this interaction, and none could explain it, for the principle itself would need to be an instinctive tendency and its interaction with the other tendencies would equally require explanation. The theory is connected with Banister's notion of the "more elementary" instinctive tendencies. If he recognised that mental activities are complex from the beginning, and are interacting from the beginning with the other activities within the mind, he would find no logical difficulty in their entering into fresh relations, and would not postulate this outside agency to relate them. Banister himself speaks of this idea of integration as an "old-fashioned" one, and though it certainly could not be said that later theories are in general better than older, still Banister does seem here, and indeed throughout the book, to be neglecting the criticisms of recent psychologists and returning to positions whose falsity they had demonstrated.

ALICE R. WALKER.

SEXUAL REGULATIONS AND CULTURAL BEHAVIOUR. By J. R. Unwin, M.C., Ph.D. (Cantab.). Oxford University Press, London: Humphrey Milford. 1935. Pp. 62. English price: 2s. 6d. net.

Dr. Unwin's address, delivered before the Medical Section of the British Psychological Society, 27th March, 1935, makes frequent reference to his earlier work, "Sex and Culture", which, he says, "contains a full statement of the evidence and conclusions summarised in this address". It was a suggestion of the analytic psychologists which caused Unwin to investigate the relationship between sex and culture, and he concludes that the psychologists were correct in making cultural progress dependent on the suppression of direct sexual satisfaction. Unwin's purpose is to supplement the evidence of the psychologists, which was based on their study of individual behaviour, by an examination of whole races, and thus to provide "cultural evidence" for the hypothesis.

The question is one of a contribution to *culture* or *civilisation,* and the significance given to these terms is of primary importance for the argument. Unwin's hypothesis neglects the fact that the state of sexual opportunity within a society is itself an index of its cultural position. It is assumed that a man's sexual life is always to be judged according to its effects on other of his activities, but just as we recognise a man's literary activities as having "intrinsic value", so it may be with his sexual activities. Thus when we examine the civilisation of a people, we shall need to look for progress in its sexual regulations as well as in other fields, and to consider this progress or backwardness as also a sign of its cultural position.

In the charts at the back of the book dealing with the cultural condition of uncivilised peoples, Unwin mentions three fields—treatment of affliction, method of weather control, and treatment of ghosts—as those in which progress or backwardness can be noted. More promising is his general statement: "From one point of view a cultural change may be described as a victory of unorthodoxy over orthodoxy, a cultural advance as a penetration of the sacred by the profane. Among uncivilised men the domain of the sacred is vast. They regard themselves as surrounded by mysterious forces or powers" (p. 6). One would expect Unwin to continue by grading peoples as more or less progressive, according as they faced unusual events with common sense and some recognition of reality, or as they depended on ritual, and that within the field of ritual activities some would be graded as more backward, some as showing recognition of at least certain facts. Thus, if as Unwin says, the deistic man regarded only certain gods and the ghosts of powerful dead men as making up the unseen powers, he might be taken as having advanced beyond manistic man who saw a power "in any stone, animal, or tree, of uncommon type or unusual appearance" (p. 9). But Unwin's grading into deistic, manistic and zoistic seems based simply on the amount of 'energy' which these people expended in their ritual relations with the unseen powers, which would imply that the race which most elaborates its ritual is the most progressive. But, as Unwin himself suggests, ritual arose as a way of dealing with certain problems, and the test of a race's progress is surely to enquire whether it is able to deal more effectively with such problems. If ritual is an inadequate solution, then progress takes the form of abandoning, not elaborating, it. In considering any division of what we call civilisation, whether that of art, science or economic life, we have to distinguish its progress from its history. Historical activities include, but are not co-extensive with, the progressive.

Such a distinction was suggested by the criterion which Unwin mentions of the penetration of the sacred by the profane, but Unwin is prevented from making any real use of it, by his misunderstanding of its significance. Speaking of the growth of a society in an expansive condition, i.e., one which is exploring and colonising, he says: "It ceases to regard normal events as normal and abnormal events as the work of the power or powers in the universe. Instead, it entertains the conception of the 'natural'. Under the influence of its mental energy it begins to understand or to think it understands, many things that hitherto have been inexplicable; and the area of the 'natural' continually expands into that which used to be occupied by the power in the universe. The profane penetrates the sacred and, though some of the most developed minds may preserve some conception of the power in the universe, based on the yet unknown, in other minds the profane alone exists. Men begin to interpret the world for themselves. Wanting a universe to suit themselves, they proceed to construct it, and soon produce one that satisfies them" (p. 22). Apparently Unwin cannot decide whether the rejection of the sacred is an advance in culture or not. Further, he has fused two mental attitudes into one. The rejection of the notion of the sacred and inexplicable, of self-explanatory powers and forces behind things, is necessary for any scientific progress. But the man who rejects the inexplicable does not therefore claim to know or to be capable of knowing everything, and does not simply proceed to a new mystification by constructing in writing a universe to suit himself. As Unwin has previously shown, it is *primitive* man who, not finding the world to suit himself, meets the situation by setting up governing powers. With these he feels he can come into some relation, such as he has failed to do with the actual distressing phenomena. Progress takes the form of coming into practical relation with the things themselves, of rejecting the notion that they are manifestations of something sacred. The man who keeps "some conception of the power in the universe" shows not his more developed mind but his failure to progress beyond primitive solutions.

It cannot be said that Unwin has succeeded in grading societies according to a satisfactory criterion, though the penetration of the sacred by the profane could be shown to apply in the fields of economic production, science and literature. Further (as was seen to be necessary for any adequate criterion of cultural progress), it is applicable in the field of sexual activities. It is not until we gain some such definite notion of the nature and direction of culture that we can profitably discuss its relation to other forces.

ALICE R. WALKER.

EDUCATIONAL, PSYCHOLOGICAL AND PERSONALITY TESTS OF 1936. By Oscar K. Buros; School of Education, Rutgers University, New Jersey. Pp. 141. 75 c.

For the third time the name of Oscar K. Buros appears upon the title page of a bibliography of Education, Psychological and Personality tests. This bibliography of tests of 1936 published as number eleven of the Rutgers University Studies in Education, has been preceded by numbers seven and nine listing tests of 1933 and 1934 and tests of 1933, 1934 and 1935 respectively. So heavy was the demand for the earlier of these works that the supply was quickly exhausted and the next number was made cumulative. The present number is non-cumulated, including only "tests published in the year 1936 and tests omitted from earlier bibliographies".

The value of this work as an aid both to test appraisal and selection and to educational and psychological research has been greatly enhanced by Buros' ability to look beyond the confines of the United States to include in this volume practically all the pencil and paper tests published as separates within the British Empire, as well as those of the U.S.A.

The dominating motive of the compilation of this bibliography has been utility. Only such tests as are readily available, and only such as are of value outside the limited testing programme for which they were constructed, are included. The information given for each test has been carefully chosen for ease of selection and purchase. Along with the title is given a description of the group for which the test was constructed; the date of copyright or publication; the nature of the test, whether group or individual; and the number of forms available. Also reported are the cost, the time required for administering, the author, the publisher, and all known references on the construction, validation, and use of the test.

The now vast conglomerate of tests, standardised and otherwise, being published each year bids fair to confuse any test user, lay or professional. A discriminating choice and use of tests can only be made possible by such a detailed survey as that of Oscar K. Buros. Both the author and the School of Education of Rutgers University are to be congratulated upon the timely appearance of this series of publications.

Still more pleasing and of even greater value to the isolated dominions reader is the appearance in this volume of a new section listing measurement books and book-review excerpts. "Books, monographs and pamphlets published in the years 1933 through 1936 on the subject of measurements and closely related subjects are listed alphabetically by authors. This bibliography of books includes practically all measurement work printed in English-speaking countries. Numerous magazines, both American and English, were searched for reviews of these books. Descriptive and critical excerpts were taken from the book reviews found and listed herein."

It should no longer be necessary for Australian students and research workers to order measurement books solely from the publisher's catalogue, nor from a single review. A concensus of opinion pro and con is made available and more discriminating choice is possible. To those whom distance prevents from examining books before purchase such a service is invaluable. The review excerpts have been chosen with discrimination; both appreciative and provocative statements have found a place. The book-review section of Educational, Psychological and Personality tests is an innovation which Australians and research workers everywhere will welcome with enthusiasm. Its annual publication will be eagerly awaited.

All information in this publication is made readily accessible by the inclusion of complete author, publisher, periodical and title indexes.

C. A. GIBB.

ABILITY AND KNOWLEDGE: THE STANDPOINT OF THE LONDON SCHOOL. By F. C. Thomas. With a forward by C. Spearman and an appendix by Elsa Walters. London and Toronto: Macmillan & Co., Ltd.; 1935. Pp. xx + 338. 15s. $4.50.

Professor Spearman's expression of pleasure at the simplicity and clarity of this work will find a ready echo in the minds of many students. Here, for the first time, is presented a readily comprehensible account of the theories and criticisms of the London School, whose principals are Professors Spearman and Aveling. "Ability and Knowledge" is an excellent and, indeed, a necessary introduction to "The Abilities of Man".

The author states as his main purpose that "this is an attempt to place before the novice in psychological studies a simple yet adequate and authoritative account of the theories associated with the name of Professor Spearman; but it also offers the more advanced student a fairly comprehensive compendium of the most important findings of what has come in recent years to be known as the 'London School'."

In addition Mr. Thomas has, in the later sections of the book, examined the chief criticisms of these findings. He has presented alternative theories as those of the Introspective school, Behaviourism, Psychoanalysis, Purposivism and Gestalt. He discusses three other theories postulating factors, *viz.*, those of Professors Thorndike, Thomson and Kelley. Finally, he seeks to estimate the value in general psychology of the "school's" work. In all this Thomas has been most successful—at least from the point of view of the more advanced student. To the novice much must still remain a mystery. The entire work is set out rather as a series of notes than as a thesis. Each major section is prefaced with a statement of the problem to be discussed; each short chapter is headed by a detailed and tabulated synopsis. Not only does this outlay make for ease of reference, but for clarity as well. Mr. Thomas apologises for his lack of any but the most ordinary literary facility. In fact, his system and logic counteract any such deficiency. The simplicity of his language is a delight.

One cannot but feel that Mr. Thomas has pointed the way as plainly as possible. Still there are difficulties, but they are inherent rather in the nature of the material than in the mode of exposition. Excellent use has been made of simple but adequate diagrams. For those who appreciate a visual presentation, even of complex material, this book does a great service. The diagrammatic representation of the Law of the Eduction of Correlates is at once ingenious and instructive.

"Ability and Knowledge" is, however, something more than a summary explanation of the theories and technique of Spearman. It is infused with the personality of its author, who has brought to this discussion not only an understanding and great respect for Professor Spearman, but also a thorough knowledge of recent developments in this field. As he has pointed out in his note, "heresy-hunting is not the vogue" in the London School; and the author is thus enabled, while still following Spearman, to depart occasionally from the strict detail of his original presentation.

It is interesting to find that Thomas adheres strictly to the "Two-factor" theory apparently without appreciating its unique position. Modern factor analysis techniques, while not invalidating the older Spearman technique, have offered a more general solution of the same problem. And, furthermore, that solution as revised by Thurstone is in practice less cumbersome. This, however, is an observation which does not affect the excellence with which this book has achieved its purpose of explaining the London School to persons who previously had encountered much difficulty with the available literature.

C. A. GIBB.

MANUAL SKILL: ITS ORGANISATION AND DEVELOPMENT. J. W. Cox, Cambridge University Press, 1934.

The work is not, as one may presuppose from the title, a monograph devoted to the problem of dexterity in general, but one describing a piece of special research. The writer, who has previously isolated the factor "m", makes an outstanding feature of statistical procedure, due care being given to every step. By applying the method of tetrad differences and partial correlations, he pursues the analysis of manual skill much further than previously dated work on this problem. The purpose of the work was to analyse the underlying factors involved in dexterity or manual skill.

Since practice enters very considerably into manual operations, especial care was taken to ensure the test groups sufficient practice to nullify random errors. Several groups were used for this purpose but one, a control group, refrained from intermediate practice except on trial occasions. Six tests of mechanical assembly were used as a basis, involving the fitting together of the parts of an electric light container, the wiring and the stripping of the completed object. A variety of other manual tests, one of general intelligence, scores in school subjects and teachers' estimates of intelligence were used as criteria.

Cox found present (1) a general factor which functions through the whole of the activities, (2) a mechanical factor whose presence he had analysed in a previous work, (3) a routine manual factor restricted to routine assembling and stripping operations, and (4) two small additional factors restricted to a pair of similar manual tests. Cox's most important contribution is the discovery of (3) a manual or dexterity factor regarded by all previous research workers as non-existent.

A. H. MARTIN.

JOURNALS RECEIVED.

MIND: A QUARTERLY REVIEW OF PSYCHOLOGY AND PHILOSOPHY. Macmillan & Co. Annual subscription: 16s.

Vol. XLVII. No. 185, January, 1938. Mill's Joint Method (II): Reginald Jackson. Some Observations on Natural Rights and the General Will (II): H. D. Lewis. Ethical Judgments and Avoidability: Charles Leslie Stevenson.

THE JOURNAL OF PHILOSOPHY. Published fortnightly. Columbia University, New York. Subscription: $4 a year.

Vol. XXXIV. No. 21, October 14, 1937. The Ninth International Congress of Philosophy and the Second International Congress of Esthetics and of the Science of Art: George Boas. The Logic of Libertarianism: H. C. Sprinkle. No. 22, October 28. Value Propositions and Verifiability: Wilbur M. Urban. Physicalism and Sensation Sentences: Norman Jacobs. No. 23, November 11. Two Traditions in the Refutation of Egoism: Abraham Edel. A Definition of the Esthetic Experience: Eliseo Vivas. No. 24, November 25. Language and Metaphysical Truth: H. G. Alexander. Beauty and Nature: Bertram Morris. The Sacramental Concept of Virtue: J. S. Boughton. No. 25, December 9. Abstracts of Papers to be Read at the Thirty-Seventh Annual Meeting of the Eastern Division of the American Philosophical Association, Princeton University, December 28, 29 and 30, 1937. No. 26, December 23. Is There Not a Clear Solution of the Knowledge-Problem?: Dickinson S. Miller.

THE PHILOSOPHICAL REVIEW. Published every two months. Cornell University, Ithaca, N.Y. Annual subscription: $5.

Vol. XLVI. No. 6, November, 1937. Economic Thought and Economic Evolution during the Last 150 Years: Herbert von Beckerath. A Convergence Theory of Similarity: Stephen C. Pepper. Knowledge and Self-Consciousness: H. W. Stuart.

REVUE PHILOSOPHIQUE. Published every two months. Alcan, Paris.

Vol. LXII. Nos. 11 and 12, November-December, 1937. Une Lettre inédite d'African Spir à A. Penjou: A. Spir. Le progrès de la métaphysique selon Leibniz et le langage des sciences: Pius Servien. Le relatif et l'actuel. En marge des pensées d'Alain: I. W. Alexander. La conversion de Renouvier au théisme: L. Foucher. Trois réfutations du parallélisme psycho-physiologique: A. Miroglio.

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Vol. XXIX. No. 5, September, 1937. Il problema della libertà in S. Agostino: Primo Montanari. La formulazione del principio di causalità e la nozione di causa in S. Tomaso: Franco Amerio. Universalità filosofica in una filosofia moderna: Frida Grosso.

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Vol. XVIII: Part 4, October 1937. Analysis Terminable and Interminable: Sigm. Freud. The Problem of the Genesis of Psychical Conflict in Earliest Infancy: Robert Wölder.

JOURNAL OF SOCIAL PSYCHOLOGY. Clarke University Press. Annual subscription: $7.

Vol. VIII. No. 4, November, 1937. Experimental Studies of Projection: II. Ideas of reference: Robert R. Sears. Emotional Stability of Chinese adolescents as measured by the Woodworth-Cady-Matthews questionnaire: C. Hart Westbrook and Yao Hsien-Hwei. Forms of personality obstructive to progress in college: Andrew H. Woods and Genevieve Chase. A Study of the extent of agreement between friends' opinions and their ability to estimate the opinions of each other: Charles Nelson Winslow. A comparative study of radio and face-to-face stimulus situations: Hadley Cautril. The interrelations of political attitudes: I. Attitudes toward candidates and specific policies: S. P. Hayes, jr.

REVUE DE L'INSTITUT DE SOCIOLOGIE. Parc Léopold. Brussels.

Vol. XVII. No. 3, July-September, 1937. La Crise Agraire (Grèves agraires et endettment de l'agriculture) (*suite et fin*): Robert-J. Lemoine. Les rapports entre la propriété et le travail dans le métayage italien: G. Jacquemyns. Le marché des annonces des journaux allemands depuis 1933: W. Eliasberg. Centres d'études sociales en Pologne: G. Jacquemyns.

NEW YORK UNIVERSITY LAW QUARTERLY REVIEW.

Vol. XV. No. 1, November, 1937. Taxation of Undistributed Profits: Paul Marcuse. Changes in the 1920 Revision of the New York Decedent Estate Law: Frank H. Twyeffort. The Implications of the Kellogg Pact with Respect to American Foreign Policy: Clarence A. Berdohl.

PACIFIC AFFAIRS. Published quarterly by The Institute of Pacific Relations. New York. Annual subscription: $2.

Vol. X. No. 4, December, 1937. American Far Eastern Policy, 1931-1937: Frederick V. Field. Japanese State Finance: Guenther Stein. Japan's "Special Interests" in China: Franz Michael. The Press and Japanese Thought: A. Morgan Young. The Phantom of Mengkukuo: Owen Lattimore. Tibet and its Neighbours: Sir Charles Bell. The Imperial Conference of 1937: F. H. Soward.

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Vol. XIII. No. 25, December, 1937. Twentieth Century Banking in Australia (Review Article): A. G. B. Fisher. Guaranteed Prices for New Zealand Exports: H. Belshaw. Incidence of Taxation: S. J. Butlin. Theory of Wage Fixation: A. Smithies. Russia, 1900-1936: A Statistical Interpretation: J. Dyason. The Fair Wage Principle in New Zealand. Its application and a Possible Extension: E. J. Riches.

THE HUMAN FACTOR. Published monthly by the National Institute of Industrial Psychology, London. Annual subscription: £1.

Vol. XI. No. 11, November, 1937. How People Compensate or Adjust Themselves for Lack of Ability: Alec Rodger. Eyesight Surveys: M. A. Vogel. Food Industry: the Investigation of Consumers' Taste Preferences: H. H. Ferguson. Commerce without Conscience. Vocational Guidance in an American College: Forrest H. Kirkpatrick. Psychological Causes of Accidents: A Critical Notice: Eric Farmer. No. 12, December, 1937. Report and Statement of Accounts for the Year 1936-7.

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FONDATION UNIVERSITAIRE. 17th Annual Report, 1936-1937. Bruxelles, 11, Rue D'Egmont.

NOTES AND NEWS.

At a meeting of the Executive of the Association held towards the end of last year it was resolved to bring out three issues of the Journal in 1938 instead of the usual four. At the same time, since each number will be increased from 80 pages to 96, the total amount of reading matter provided in the year will not be greatly reduced. This change of policy which has been necessitated by the recent rise in the cost of printing, will involve a small saving to the Association on the year's work. The Executive believes that it will have the approval of members in this matter, and hopes that the support of members during the year will put it in a position to revert next year to quarterly publication.

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The Annual Congress of the Association will be held this year in Sydney on May 19th and 20th. A number of Sydney students attended the Congress in Melbourne last year, and it is expected that on this occasion students will be included amongst the visitors from Melbourne. Speakers from Brisbane and New Zealand are also expected. All members of the Association are invited to attend the meetings of the Congress. The programme is not yet available for publication, but a leaflet will be posted to members within the next few weeks.